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OF

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IN SCOTLAND

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1883-84.

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INSTITUTION OF ENGINEERS & SHIPBUILDERS IN SCOTLAND.

(INCORPORATED.)

TWENTY-SEVENTH SESSION—1883-84.

Introductory Address. By Mr JAMES REID, President.

Read 23rd October, 1883.

GENTLEMEN—

When I had the pleasure of addressing you from this chair last year I craved your indulgence and support in the discharge of my duties as President. With your kind co-operation I have, I trust, in some small measure justified your selection, and I shall, trusting in the like manner, begin my present address by giving a cursory account of my stewardship during the past session.

And here it may be remarked that the discussion of a paper is not the least valuable part of our work as a society; much valuable information being in this manner elicited from members who often cannot, from their various duties, see their way to favour us with papers.

Short suggestive papers, therefore, are very valuable and much to be desired, as they in this manner stimulate discussion.

The papers that have been read and discussed in the course of the session are as follows, viz.:—

The discussion on Mr Hogg's paper "On St. Enoch Railway Station" was taken up at our first meeting in October, the paper having been contributed at the closing meeting of the previous

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session. For this paper Mr Hogg has been awarded the "Railway Engineering Medal."

A paper by Mr Laurence Hill, C.E., "On the Cultivation of Railway Embankment Slopes," was read on the same evening and discussed.

At the November meeting a paper was read by Mr James Reid, "On Tyer's Train Describer," which he described as a combination of electric bell apparatus and indicating dials, by means of which the business of six platforms with eight lines of rails, at St. Enoch Station, was regulated.

On same evening a paper "On Reversing Valve Gear," by Mr Alexander Morton, was devoted to one of a class of valve gears which was a partial reversion to forms of gear brought forward in the early days of the locomotive, in which the motion was derived from the lateral movement of the connecting-rod: superseding the employment of eccentrics and link-motion. The author describes the means for converting the irregular lateral motion of the connecting rod into a regular motion, with other refinements for insuring equal distribution of steam with a quick cut-off.

A paper "On Water Wheels and Turbines," by Mr John Turnbull, Jun., was notable as the first paper read before us on the subject of the utilization of water-power. The author describes the testing works at Holyoke, Massachusetts, and gives a full account of the Hercules Turbine, the type of turbine which he selected and erected at the works of Alexander Pirie & Sons, Aberdeen. He contrasts the large water wheel, 72 feet in diameter, at the Shaws Water, Greenock, weighing 115 tons, and costing £3000, with a Hercules Turbine of equal power, 15 inches in diameter, and weighing half a ton, and costing £50.

A paper "On Electric Lighting for Workshops and Large Spaces," by Principal Jamieson, was a valuable original contribution which gave rise to an instructive discussion on the cost of lighting by electricity.

The sixth paper read during the session, "On an Expansive Engine reversing by Valve," by Mr Robert Duncan, described an effective

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means of working the system of reversing valves with economy in the adoption of compound cylinders, the valves and cranks being so arranged that the work of four eccentrics, required in ordinary systems, was performed by one eccentric. The reading of the paper was followed by an interesting discussion.

Mr L. J. Groves read a paper "On the Exhaust Steam Injector," describing an interesting and, one may say, an unexpected development of the principle of the injector, by means of which not only is there a positive saving of steam in the operation of the instrument, but there is also a gain of effective power by a reduction of back pressure on the pistons of non-condensing engines. A natural head of feed-water is necessary for the action of the apparatus. A discussion followed.

At the meeting in March a paper was read by Mr James M. Gale, of the Glasgow Water Works, "On the Latest Additions to the Loch Katrine Water Works." This is virtually a continuation of the paper he read before the Institution in 1863. The population in 1863 was 350,000, supplied with 40 gallons per head per day. In 1882, the population was more than double, amounting to 775,000, and was supplied with 54 gallons per head per day. The total cost up to May, 1882, was about 2¼ million pounds sterling, the water supply extending over an area of 15½ miles north and south by 11 miles east and west, and requiring about 350 miles of piping. In the paper much consideration is given to the waste of water, a question of prime importance, seeing that by a system of competent inspection the consumption in a given district in Liverpool was reduced from 49 gallons per head per day to 32 gallons. The sufficiency of the fittings, it is properly pointed out in the paper, is indispensable. An interesting discussion followed, bearing mainly on the arrangement of distributing pipes, and formulæ for measurement of flow of water.

At the Annual Meeting of the Institution, held on 24th April, Mr Joseph Moore, C.E., of San Francisco, favoured us with a paper "On Malleable-Iron Piping of Large Diameters for Conveying Water," giving a brief notice of the process of gold mining in

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California, and describing the construction of the wrought-iron water pipes, some of which are from 30 to 42 inches in diameter, and constructed of sheet iron of from No. 14 to No. 9 wire-gauge. The discussion of this paper was deferred until the opening meeting of the present session.

I may state here that, in addition to the medal awarded to Mr Hogg for his paper, the "Marine Engineering Medal" was awarded to Mr James Hamilton, Jun., for his paper on "The Comparative Commercial Efficiency of some Steam Ships," and the "Institution Medal" to Mr Joseph Moore, C.E., for his paper on "Hydraulic Machinery for Deep Mining." These papers were, as you may be aware, read during the course of the previous session.

The success of all societies such as ours depends mainly on the subject matter considered and discussed, and, I think, that in looking over the Roll of Membership we may congratulate ourselves in having such a number of gentlemen who are so well qualified, from their position and experience, to enrich the literature of our Institution by contributing to our meetings papers upon subjects with which they are intimately conversant. It is by no means necessary, nor is it in many cases advisable, to write long papers, short suggestive papers frequently giving rise to interesting discussions whereby much important information is elicited.

I am glad to state that our last session was a very prosperous one, a considerable addition having been made to the Membership. We have now on the roll—

Honorary Members, - - -	7
Life Members, - - - -	10
Ordinary Members, - - -	372
Associates, - - -	38
Graduates, - - - -	154
Total, - -	581

The addition to the Membership during the session has been 25 new members, 4 new associates, and 35 graduates, or a total of

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64 gentlemen elected into our Institution. This, I think, is a very satisfactory indication of the position which the Institution is now recognised as holding amongst those best qualified to judge.

When congratulating ourselves on the accession to the membership of the Institution we cannot pass over in silence the losses that have occurred by death since we last met. Two most prominent members (Past Presidents of the Institution), viz., Messrs John L. K. Jamieson and R. Bruce Bell, C.E. Both of the deceased were so well-known and took such an active part in the affairs of the Institution that I need only refer to the great loss we have sustained by their decease.

When I addressed you last year I drew your attention to the development of railway traffic, and to the beneficial results of lower fares and increased facilities in not only wonderfully augmenting third-class traffic, but also in adding, upon the whole, to the receipts of railway companies. The increase in receipts from passengers in the year ending June 30, 1882 is wholly due to the increase of third-class passenger traffic — a most gratifying proof of general prosperity in the country. The total increase in passenger receipts was at the rate of four per cent. as compared with the receipts of the previous year. There was a deficiency in first and second-class receipts, amounting to £70,000, and the actual rate of third-class increase was nearly 7 per cent. amounting to more than a million sterling.

Reverting to the case of the Midland Railway Company, whose operations I noticed last year, they have kept pace with the general advance. In the year 1880-81 there were 29 millions of travellers on that line, but in 1881-82 there were 30½ millions, showing an increase of 1½ millions in the year. The whole of this increase is due to the growth of the third-class traffic, which reached over 28½ millions in 1882, and was 1½ millions more than in 1881, whilst a smaller number of first-class passengers were carried in 1882. The receipts from passenger traffic at the same time were increased by £100,000 in 1882, as against 1881.

There has been, it is unquestionable, a gravitation of traffic to the

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lowest level, aided by the fact that third-class carriages have been for some years run with most trains, and that the largeness of the number of third-class passengers has forced upon the management of the companies improvements in the popular class of carriage. As the downward movement of classes is still continuing, the outcome will most likely be a general reduction of the number of classes to two; nominally first and third, practically first and second.

In contrast with such a refinement of civilisation may be noticed the Northern Pacific Railway, which makes a line of 1919 miles across the Continent of North America, between the Mississippi River in Minnesota and the Pacific Ocean in the State of Oregon, the remotest State in the Union. On the 22nd August last, the last rail was laid, uniting the two ends of the railway near Helenas Mountain, on the western slope of the Rocky Mountains, but the opening was postponed until the official driving of the last spike was performed, which took place on the 8th September.

The country is comparatively new, rich in minerals, and with boundless possibilities of agricultural wealth. It is almost inevitable that, in these distant portions of the United States, railways should precede population, and it is impossible to contemplate the opening up of these great and fruitful territories without a feeling of content that many of the problems which seem to threaten society in the crowded countries of the old world, may find their solution in the ample room thus found in the new world for energies which are cramped or wasted here.

But our railways at home have a useful work yet to do in circulating food supplies. The Fish League have had constructed refrigerator cars, which have been established to improve the system of fish collection and delivery. They are constructed to carry five tons of fish, being ventilated by a fan or blower worked from one of the axles of the car. The Fish League have already six cars in work between the Scotch ports and London, and they intend to extend their operations to Ireland.

The Parcel Post is another recent development of railway facilities. Though for business purposes the use of the new organization seems

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to have come out a good deal below what was commonly expected in strictly business circles, there can be no doubt that the general public throughout the country have taken to the parcel post with avidity. The general delivery throughout London advanced from 5000 parcels on the second morning of its existence, to 12,000 in about three weeks.

The Glasgow City and District Railway, now in course of construction under the direction of the engineers, Messrs A. Simpson & Wilson, will supply important links of communication between the railways on the north side of the river. The line extends from College Station, High Street, by George Street and Regent Street, crossing Dumbarton Road, to the existing Stobcross line, over a length of nearly 2½ miles, almost wholly underground. Of this, 1700 yards, or nearly one mile, are tunnelled, and 1000 yards are covered way. The tunnels are carried through boulder clay, meeting with occasional pockets of running sand and mud, and through sandstone and minerals.; they are arched with four rings of brick in cement, to a clear height of 18 feet 6 inches at the crown, and 27 feet in width, for two lines of way. The covered way is arched with brick. The new line when finished will, together with a short piece of line between Annisland and the present Helensburgh Railway, open up a direct route to Dumbarton and Helensburgh clear of the Queen Street incline. Moreover, by the aid of connections to the north and east, a circuit line of about twelve miles in length will be completed from College Street, by Garngad Hill and the existing Helensburgh line, by Annisland to Partick, returning by the tunnelled way to College Station. The capital for the undertaking is £550,000.

A new departure in locomotive practice has been taken, in compounding the cylinders—in France, by M. Anatole Mallet; and in England by Mr F. W. Webb. M. Mallet simply compounds the two cylinders by differentiating them. He successfully started his first compound-cylinder locomotive on the Bayonne and Biarritz Railway, in July, 1876. The cylinders were 9½ inches and 15¾ inches in diameter, with a stroke of 17¾ inches. The steam, of

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140 lbs. per square inch pressure, was cut off in each cylinder at 60 per cent. of the stroke, expanding nominally 2½ times. In engines subsequently built, means have been provided for cutting off the steam independently at different points of the stroke in the two cylinders.

Mr F. W. Webb, in 1878, had compounded a locomotive at Crewe by lining one of the cylinders; and more recently, in 1881, he constructed the engine "Experiment," with three cylinders on the compound system, two 11½-inch outside cylinders exhausting into one 26-inch inside cylinder. In engines subsequently designed, he employs 13-inch high-pressure cylinders. The outside cylinders are connected to one pair of wheels, and the inside cylinder to another pair, each pair of wheels working independently without coupling-rods. The results of performance are stated to be very satisfactory, but the maximum practical economy can only be attained by the use of steam-jackets on the cylinders, or of superheated steam.

A new light has in recent years been shed on the utility of tramways in town and country, partly as a means of communication auxiliary to railways, and partly as independent means of locomotion in streets and roads. A tramway, in the modern sense of the word, is a street railway or a road railway, forming part of the road or the street, and constituting with the carriage-way a combination of railways and common thoroughfares such that the traffic of the street or road, unaffected by the tramway, is free to circulate. It follows as a condition of such free circulation, that the surface of the rails, so long as these are formed with grooves for carrying flanged wheels, should be at the level of the carriage way.

The modern tramway was introduced into England by Mr G. F. Train, who in 1857 made proposals for laying tramways on the step-rail system originated in Philadelphia, in some of the metropolitan thoroughfares, and in a few provincial towns. Lines were laid in a few places, but after brief periods of trial, the lines were removed, though in some places, as Birkenhead, flat grooved rails

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were substituted for the step-rails. The first English system of tramways for passenger traffic that was authorised by Act of Parliament, is that of Liverpool, for which an Act was obtained in 1868. The works were constructed under Mr George Hopkins, as engineer-in-chief, to a gauge of 4 feet 8½ inches The rail was little else than a flat bar of iron, having a narrow and shallow groove in its upper part, with a fillet on the lower side, bedded on a longitudinal timber sleeper. The joints of the rail were fixed with four vertical spikes, two to each rail, driven into the sleeper. This system, though it was imitated elsewhere, was of a fatally weak construction, and has now generally been abandoned. The design of the Glasgow Corporation Tramways, for which the first Act was obtained in 1870 by a private company, was a distinct advance on what had previously been done. In designing the first of the Corporation Tramways, Messrs Johnstones & Rankine, the engineers, developed the employment of the transverse sleeper for giving bearing surface and for strength. They adopted a modified form of the old flat grooved rail in combination with longitudinal and transverse sleepers and bituminous concrete, and produced a structure which surpassed all others that had previously been laid for tramways, in strength and solidity. A curious question arose with respect to the gauge of rails, which, according to the Act of 1870, was required to be equal to the railway gauge—4 feet 8½ inches. A new element was introduced by the Act passed in 1871, for the Vale of Clyde Tramways, according to which their lines were to be constructed for the transit of railway rolling stock, which should also run over the Corporation Tramways. In order, however, that the passage of such stock should be practicable, the gauge of the tramways must be less than the normal standard gauge; for, what is strange at first sight, the gauge of 4 feet 8½ inches in a grooved rail does not answer for railway waggons. The reason becomes apparent when it is considered that the groove is not wide enough to clear the backs of the tyres. These are not more than 4 feet 5½ inches apart, a width which is 3 inches less than the normal gauge, equivalent to 1½ inches at each rail. As the width of the groove in

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the tramrail is commonly limited to 1⅛ inches, or at the most to 1¼ inches, the flanges of waggons would bind, and could not in fact enter at all. On the initiative of Messrs Johnstones & Rankine, the gauge was contracted by three-quarters of an inch—to 4 feet 7¾ inches—maintaining the minimum width of groove already adopted by them—1¼ inches. The inner sides of the groove thereby acquired a total lateral clearance of ½-inch between the backs of the tyres and the inner edges of the grooves; and that the wheel-flanges should not wedge themselves into the groove, the depth of the groove was limited to ¾-inch, so that the wheels would run on their flanges at the bottom of the grooves. A few preliminary trials were made, and the results of the trials led to the adoption, by general consent, of the gauge of 4 feet 7¾ inches for all the tramways of the Glasgow district

In designing the newer lines of the Glasgow Corporation Tramways, Messrs Johnstones & Rankine, with the prospect of the future employment of steam-power for traction, and the attendant heavy rolling loads, adopted steel for the material of the rails of the new lines. The rails are girders 6½ inches high, and have a flange base 5½ inches wide, rolled in lengths of 24 feet, and spiked down to cross timber sleepers imbedded in concrete. The new system of tramway costs, for the way alone, £3711 per mile of double line; for paving, £4664 per mile; making together £8375 per mile of double line.

Though the modern tramway is but of recent origin—not fifteen years old—there were in the United Kingdom at June 30, 1882, about 1000 miles of tramways authorised to be constructed, of which 564 miles were open for passenger traffic; about half of the length double line, and the other half single line. Of this quantity there are 80 miles in the metropolis, 43 miles in Manchester, 13½ in Liverpool, 14½ in Edinburgh, 28½ miles in Glasgow, and 40 miles in Dublin. The total capital expended on tramways at June 30, 1882, amounted to about 8¼ millions sterling, being at the rate of about £14,600 per mile open.

Unassuming and unobtrusive as they are, tramways, it is seen,

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cost half as much as railways; and they actually earn about as much money by the mile open. The gross receipts for the year ended June 30, 1882, amounted to about two millions sterling, being at the rate of about £70 per average mile open per week, against £72 per mile per week on railways. Taking tramways individually, the North Metropolitan earned £166 per mile open per week, and the Glasgow Corporation Tramways earned £165 per mile per week. Against these sums the London and North-Western Railway earns £112 per mile open per week. The Metropolitan Railway, on the contrary, earned £560 per mile per week.

Unfortunately, the working expenses of tramways are high. They average 79 per cent, or about four-fifths of the receipts. Seven or eight years ago they were only 75 per cent. of the receipts. This is the weak point of tramway industry. Tramways are indisputably of public benefit, conferring the greatest degree of convenience on the greatest number, but they have got to settle down to a lower margin of working cost. The item of cost for horse-power is by far the largest—it is over 6d per mile-run, or more than half of the gross expenditure.

The largeness of the cost for haulage by horses has led to the employment of steam power as a substitute. The experience of steam power has not yet been of sufficient duration to test the cost averaged over a long period. On the Stockton and Darlington Tramway, the total cost for engine-power is, according to the report last issued, 3·12d per mile run. In contrast with this, the cost of engine power on the Rouen Tramways in 1882, under the management of Mr J. Arthur Wright, was 10·16d per mile run. The average cost for repairs alone, including the re tubing of fifteen out of sixteen engines, was 3½d per mile-run. The road is in a bad state, and Mr Wright is preparing contracts for relaying the steam-lines of way, and newly designed engines and cars to work on them. On the Northern Tramways of Paris, the repairs alone have cost 8d per mile run. But, in nearly all the cases of failure of steam traction, it is the road that is in fault. Given good roads, and suitable engines, there need be no doubt that horse-power can be superseded

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by steam-power. A few attempts at the employment of engines worked by compressed air have been made; the system has not yet made its mark; the cost of fixed plant in addition to the motors is against its introduction.

I may state that a car was made at Hyde Park Locomotive Works for propulsion by compressed air, and when tried on the Sighthill Branch of the North British Railway, the reservoirs being charged with air at a pressure of 350 lbs. per square inch, and the car being loaded with weights equal to 40 passengers, a distance of 8½ miles was run with one charge of air.

Electric power has recently been applied for propulsion on railways. A tramway a mile and a half in length has been worked by electricity for two years in Berlin; and a narrow-gauge line, between Portrush and Bushmills, in the north of Ireland, six miles in length, and worked by electrical power, was opened for traffic on the 28th September last. The line is single, and has a gauge of 3 feet. The gradients are very heavy, having a slope of 1 in 35 at many parts. The curves are very quick, following the line of the road. The undertaking has been carried out under the direction of Dr Siemens. The system of working is nearly identical with that adopted at the Crystal Palace, except that, instead of the rails being employed as conductors, a separate conductor is employed, consisting of a third rail, weighing 19 lbs. to the yard, and laid close to the fence. Electricity is transmitted through the conductor to the Siemens motors by which the car is propelled. The dynamo machines, by which the electricity is generated, are driven by the power of a natural waterfall of 26 feet, on the line of the river Bush. Two turbines are driven by the fall of water at a speed of 225 revolutions per minute, and capable of each yielding 50 H.P.

The rope haulage system of tramways has been carried out on a large scale in America, and is, I believe, to be tried in a part of London. In San Francisco, Chicago, and elsewhere in America, the system adopted is that of an endless steel wire rope supported on sheaves; the whole is contained in an iron tube placed beneath the roadway. A narrow slit of about an inch wide is left in the

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upper part of the tube, through which an arm passes and is attached to the car. By means of this arm the rope can be gripped tight or slackened at will by the driver or attendant in the car. The rope is kept in motion by a powerful steam engine. In San Francisco some of the ropes are three inches in circumference, and 11,000 feet long; the sheaves upon which they rest are 11 inches in diameter, placed 39 feet apart. In Chicago some of the ropes are 27,000 feet long. Great economy is claimed for this method of tramway haulage.

The anticipation of one of our members, referred to in my address of last year, that the steam-ships of the immediate future will have a length of 600 feet, with a beam of 80 feet, has yet to be realised. Steam shipping is nevertheless advancing. The "Alaska," of the Guion Line, arrived at New York on the 23rd September last, after a run of 2784 miles, in 6 days, 21 hours, and 40 minutes, probably the fastest voyage on record, having been made at the average speed of 17 miles per hour. The "Oregon," another ship of the Guion Line, also built by John Elder & Co., on lines similar to those of the "Alaska," has been designed to combine greater speed with stability. Her length is 520 feet, her breadth 54 feet, and her depth 40 feet 9 inches, and she is provided with engines and boilers of unprecedented power, which have already been proved up to 12,000 indicated horse power. There are three cylinders compounded, so that the steam from the 70-inch smaller cylinder is exhausted simultaneously into two 104-inch cylinders, all having 6 feet of stroke. The speed attained on the occasion of the recent trial was 23 miles per hour. The furnaces are constructed of corrugated plates, bent in the direction of the corrugations, and welded at the seams, the invention of Mr Samson Fox of Leeds. The resisting power of the plate to collapse is greatly increased by corrugation, and thinner plates may be used than such as are necessary for plain flues. No very definite accounts of comparative tests for evaporative efficiency have been published, but it is likely that the corrugations are favourable for the more rapid absorption of heat than plain surfaces, in virtue of the impingement of the flame current on the corrugations.

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Shipbuilders are giving considerable attention to the realization of an express Atlantic service. At present American liners spend nearly as much time in port as on the Atlantic. Steamers devoted wholly to the rapid carriage of passengers would need to spend only one-fourth of their time in port, and would make double the number of journeys made by steamers that load and unload cargo. Although 10 tons of coal per hour would be consumed for the high speed intended, the cost of the extra coal consumed for the high speed becomes a comparatively small item when placed against the large passenger freight and the fact that the fast vessels are always full. At an average speed of 19¼ miles per hour the Atlantic could be traversed in six days.

In docks and harbour works, the most notable event of the year is the passage through Parliament of the bill for the construction of docks and other works at the harbour of Glasgow. The scheme, is estimated to cost about 1½ millions sterling, exclusive of land. The works are to consist of two large docks or tidal basins, and a couple of graving docks attached, on the south side of the harbour at Govan, and an extension of Yorkhill Wharf on the north side up to Kelvinhaugh. Compared with the Queen's Dock, the proposed docks have a total area of 105 acres, against 63 acres of the Queen's Dock, 38½ acres of water surface against 33¾ acres, 3786 lineal yards of quayage, against 3334 lineal yards, 40 acres of quay space against 27½ acres, 11 acres of shed against 6½ acres.

The bill for the Manchester Ship Canal was thrown out last session by the Committee of the House of Lords, but it is to be brought in again next session, and the promoters are confident of ultimate success in obtaining the bill. The scheme has all the points of a first-rate speculation. The industries of Lancashire are in danger; her mercantile community can find all the capital required; in one industry alone, the cotton trade, half-a-million a-year can be saved in cost of transit; the staple articles of food cost at present five per cent. more in Manchester than they cost at the seaport. But a wider question is brought to the front than that of the Manchester Canal alone. In order effectively to compete with rail-

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ways by water communication the whole inland navigation of the country must be taken in hand, and there is this to be said specially in favour of canal communication, that the canals can be flanked at all points with factories.

The difficulties encountered by English shipping in the Suez Canal, and the apparent hopelessness of getting them adjusted or removed, have naturally turned the attention of British shipowners to alternative plans of maritime communication between the two seas. The Associated Chambers of Commerce have passed a resolution that another and independent canal should be cut across the Isthmus of Suez. But the Mediterranean has a long seaboard in the Levant, and the Red Sea has two branches at its northern extremity, of which one is occupied by the existing canal, and the other is open to all the world. There exists also a long and deep depression which lies for the most part many hundreds of feet below the level of the two seas, and is occupied by the Dead Sea and the valley of the River Jordan. If the Mediterranean could be induced to flow into the Jordan Valley, a new and magnificent channel would be provided, with but 108 miles of land intervening. Given unlimited capital, commanding an indefinite number of wheelbarrows and water conduits, and the thing required, the Jordan Canal, is as good as done. Nazareth, Damascus, and Jerusalem would become seaports, whilst Jericho and Tiberias would lie 200 fathoms deep. But the proposed route would be several hundred miles longer than the existing route, and the prime question remains, would it pay?

Until the very satisfactory financial results of the Suez Canal gave evidence that merchants and shipowners were willing to pay handsomely for the use of a route which would materially shorten long voyages, the scheme of the Panama Canal for uniting the Atlantic and Pacific Oceans across the narrow isthmus, did not appear to be financially practicable. It is now in course of construction—a result of the wonderful energy of Ferdinand de Lesseps—without locks, from deep water to deep water, for a length of 45½ miles, 28 feet in depth, and of a minimum width of 72 feet. (From later information that I have received, it appears that locks will be

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necessary, as at Colon in the Atlantic there is very little tide; whilst at Panama, on the Pacific Coast, the tide rises and falls 13 feet on an average, and in extreme cases 20 feet. Locks will therefore be constructed at the Panama end, having a depth of about 30 feet below low-water mark. The additional cost is £460,000.) The Canal is provided with passing places. It commences at Colon, in the Bay of Limon on the Atlantic Coast, passes through the marshes of Mindi and joins the River Chagres, which is cut by it at several places. At Matachin it leaves the course of the Chagres, passes along the Valley of the Obispo, and by the Valley of the Rio Grande it reaches the Gulf of Panama near the Islands of Naos and Flamenro. The estimated total cost of the canal is £31,200,000. It will abridge the voyage between Europe and the Western Coast of America, at the equator, by some 2500 marine leagues, and will considerably shorten the voyage to Australia, New Zealand, China, and Japan. Colon is a town built on a low coral rock standing three or four feet above the level of the sea. The aspect of the ill-kept wooden houses, the disorder of the streets, the pools of foul water, the debris of burnt refuse, the odour of the natives, excited a disagreeable impression. The departing steamer was regarded with regretful gaze, and already the newly arrived traveller was afflicted with nostalgia. When the pioneers landed they found the project in a state of chaos. Operations for the construction of the canal were commenced in 1881. The whole of the ine from Colon to Panama was surveyed, sites were selected, villages built, and the work was apportioned in twelve separate contracts, which were and are carried out simultaneously. In two years and a half, the surface has been, for its whole length, cleared of enormous trees, brushwood, and creepers, on a breadth of 300 yards. There are now about 8000 men employed along the line of works, locomotives are at work, 2000 waggons are in use, also excavators, 15 in number, steam cranes and other appliances. At the port of Colon the Company employ nearly 3000 workmen; they erect and launch tugs, dredgers, lighters, steam-pinnaces, boats, cranes, and other machinery. A fine marine dredger, erected by Messrs Lobnitz

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& Co., Renfrew, has been employed scooping out mud at the mouth of the canal. The work of excavation has commenced, and, in the next dry season, 15,000 workmen will be engaged in piercing soil and rock. The canal, it is anticipated, will be open for traffic before the year 1890.

The Channel Tunnel, under the Straits of Dover, is one of the most important projects of the age. Upon the basis of researches made under the direction of Sir John Hawkshaw, as to the nature of the strata on both sides of the channel, and of the underlying sea bed, an Act of Parliament was passed in 1875, and a concession was granted by the French Legislature to a French company, authorizing certain preliminary works for the undertaking—the effect of these researches being that they fixed a site for the French end of the proposed tunnel about 7 miles west of Calais. The progress of this scheme has, for a time, been arrested by the action of Parliament.

The Forth Bridge, which is to be erected across the Forth at Queensferry, will be the largest bridge yet constructed. It has been designed by Mr John Fowler and Mr Benjamin Baker, who are carrying out the work. The bridge will have a clear headway of 150 feet for navigation. The two central spans are to be each of 1700 feet in width, each of them consisting of two cantilevers, 675 feet long, with an intermediate girder, 350 feet long. The central pier, on the island of Inchgarvie, will consist of four cylindrical masses of concrete and masonry, 70 feet in diameter at the base, and 45 feet at the top. The other spans, twenty-two in number, are of less width, and the length of the bridge will be 5330 feet, or more than a mile; and that of the viaduct approaches, 2754 feet—making, altogether, upwards of a mile and a half in length, at the contract price of £1,600,000, or nearly £200 per lineal foot.

Another type of bridge is the East River Bridge, between New York and Brooklyn, recently opened for railway, street, and foot-passenger service, being the largest bridge yet constructed on the suspension principle. It is 85 feet wide, and has a clear height of 135 feet above water, and about 6000 feet in length, in three spans,

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of which the middle span is 1595½ feet. There are four cables 15¾ inches in diameter. The suspension towers are 278 feet above high water. The cost is about £2,800,000, irrespective of the cost of land, being at the rate of £467 per lineal foot. This rate of cost is more than double that of the Forth bridge; but the American bridge has probably more than double the width.

The employment of hydraulic power in engineering service is being extended to new purposes. Just eighty years ago, Bramah, describing the hydraulic press named after him, said that he would engage to supply power over the whole of Dublin, from a single central engine, through mains. Seventy years passed before the first attempt was made at Hull to supply hydraulic motive power from mains. A first experimental test is shortly to be made at a large warehouse by St. Saviour's Dock, London, worked by hydraulic power from an accumulator, a mile distant, at Holland Street, Blackfriars Bridge. The accumulator is a 20-inch ram, having a rise of 23 feet, with a pressure of 750 lbs. per square inch. The pressure is transmitted through ten miles of 6-inch pipe, on both sides of the river Thames. Messrs Elkington & Woodall are the engineers of this power supply. The comparative economy of the hydraulic accumulator service is exemplified in the case of a firm which spends £160 a year on its own engines and lift, and is to be supplied from the accumulator at Blackfriars Bridge at a cost of £45 for the same lifting service. The application of hydraulic machinery —or water-pressure machinery, as it is sometimes called—for working drawbridges, for dock service, for working cranes, hoists, and lifts, and presses, is familiar to engineers. But for the first time in carrying out dock works, the excavation and masonry of the Alexandra Dock works, at Hull, have been executed, in great part, by means of hydraulic machinery worked by fixed engines of 300 H.P., having a 20-inch accumulator with a 35 feet stroke. This service is extended over a water-area 2300 feet in length, and 1000 feet in width.

The transmission of power by electricity is entering into the domain of practice. It is now thirteen years since Gramme, in

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1870, invented his machine by which a continuous and nearly uniform current of electricity is produced in an economical manner. The invention consists in binding coils of wire upon an iron ring, and rotating the ring between the poles of a powerful magnet, bearing the relation to each other of armature and field magnet. The current is drawn off from the wire of the coils by two brushes which rub against pieces of metal connected with the coil, forming the commutator. Another class of inventors, including Siemens and Edison, besides others, have discarded the Gramme ring, and substituted an iron cylinder having wire wound upon it, and a commutator like that of Gramme's machine. The most important installation for the transmission of motive power at the recent electrical exhibition at Vienna, was the electric railway of Messrs Siemens & Halske. The current of electricity is generated by two compound dynamos of large size, in which the coils for magnetizing the machines are so wound that a large or a small quantity of electricity may be taken off for use in the circuit, and the electro-motive force does not vary. The current is carried from the generators by wires to the rails upon which the cars run. The successive rails are connected at the ends by strips of metal soldered to them. The current is taken up by the wheels of the cars, and is so transmitted to the dynamos, which rotate under the influence of the current, and cause the wheels to revolve by means of wire-rope gearing. The line is one mile in length; the engines and dynamos for generating electricity are at one end; and the speed of 20 miles per hour is attained. Two dynamos are employed to generate electricity, each of them using 25 H.P. Two large tram-cars, together, are propelled by the current thus generated, and they frequently carry 100 persons, there being developed 12 H.P. in each of the two motors. When 100 persons can be carried one mile by the consumpt of five shillings' worth of coal, the problem approaches, at least, the practical stage. A concession has been granted to Messrs Siemens & Halske for laying a system of metropolitan electrical railways for the town of Vienna. It will consist of a series of circuits in the centre of the town, connected by an outer circuit.

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The electrical tramway in the north of Ireland, now working, I have already noticed; and successful beginnings have been made at Litchenfeld, in the mines of Saxony and La Perroniere, at Charlottenberg, and other places.

The Electrical Power and Storage Company have excited interest by their electric launch, fitted up by them in conjunction with Messrs Yarrow & Co., and sent to the Vienna Exhibition. This launch is 40 feet long, with a beam of 6 feet, and has a two-bladed screw, 19 inches in diameter; it can carry 40 passengers. The moter is a Siemens machine, directly connected to the propeller shaft, and developing 7 H.P. with 80 cells. The motor and batteries together weigh 2¼ tons. The speed, on trial, was 8 miles per hour.

Mr J. Clarke, of Glasgow, describes a wooden launch, 21 feet long with 4-feet beam, drawing 12 inches of water with three or four persons on board. She is fitted with batteries which are charged every four hours. During several trials at Kilcreggan, on the Clyde, a speed of over 5 miles per hour was maintained.

The Electric Power and Storage Company exhibit, at Vienna, a tricycle which has four cells in two boxes, one before and the other behind the axle. The Faure-Sellon-Volkmar accumulators are used, consisting each of a glass cell containing twelve pairs of plates, 1 foot square and ¼ inch thick.

As electricity can be stored, it is unnecessary to employ a large engine, with power equal to the demand made upon it, for, say, four hours out of the twenty-four. On the contrary, it was discovered very quickly that a small engine, working for the whole day, and storing electricity, would do the same quantity of work as the large engine, and at much less expense. As to the original force employed to generate the electricity, the cheapest at hand is the best: whether that of gas, of coal, or of water. Sir William Armstrong has, for a long time, illuminated his picture gallery at Craigside by electricity produced by the stream which runs through his grounds. But what is of present interest is the valuable outcome of the electric accumulator from the determination of the scientific mind towards the employment of electricity as a motive power.

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The experiment of supplying electric light on the principle of the gas supply will be tried within the next year or two in a very large number of places. It is not yet clear how large an area can be supplied from a single centre. Engine-rooms and places for machinery will have to be set up in proximity to important business quarters and expensive residential neighbourhoods; and the machinery must be at work in the night. These conditions may prove serious causes of delay. The price at which the current can be supplied has not yet been definitely ascertained. Many men of experience believe that the electric light will never be able to compete with gas in point of cheapness: some think otherwise.

The incandescent electric lamp is, in fact, an ideal mode of domestic lighting. It combines every requirement of healthy household illumination. There are other uses for gas more advantageous than that of lighting the rooms in which we live; and we are fortunate in the prospect of seeing the new mode of illumination brought within our reach.

Gas has, in recent years, been extensively used for cooking and for heating rooms. At the Smoke Abatement Exhibition at South Kensington, in 1882, many tests were conducted by Mr D. K. Clark, C.E., the engineer of the exhibition, of cooking stoves and heating stoves and fireplaces. Though heating rooms by gas was found to be more costly than heating by coal fires, cooking by gas, on the contrary, was cheaper than cooking by coal. The Dowson gas, moreover—a gas generated from anthracite by means of combined currents of superheated steam and air—costs only about half as much as coal gas for the generation of motive power in gas-engines, which are rapidly superseding steam engines of small power.

An exhaustive course of tests of gas for lighting, cooking, and heating has just been completed at the South Metropolitan Gas Works, London. The extending employment of gas for heating and cooking offers, so far as it goes, an effectual remedy for the smoke nuisance from domestic fires; but, for heating, the cost of gas is excessive. According to the results of the tests at South Kensington, the cost for gas, at 3s per 1000 cubic feet, was four times as much as that for coal at 20s per ton, in doing the same duty.

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Meantime, the nuisance of smoke from domestic fireplaces sensibly diminishes, as improved systems of open grates and stoves are introduced. It was clearly established, by the results of the tests at the South Kensington and Manchester Smoke Abatement Exhibitions, that by passing the fresh gases and fresh products of combustion of coal through the incandescent portions, or the burning coke, smoke was considerably abated; although, it must be added, the heating efficiency was not augmented. But the problem of a smokeless domestic fire with maximum efficiency is being steadily worked out by the National Smoke Abatement Institution, the outcome of the South Kensington Exhibition.

The problem of complete combustion without smoke, likewise, is advancing towards a general solution. Mechanical stokers of the more recent descriptions have been worked with success. They are of two classes, analagous to ordinary coking-firing, and spreading firing by hand. Hitherto, the coking systems of mechanical stokers have yielded the most satisfactory results in smokelessness and general behaviour. Furnaces in which the fuel is introduced on long inclined grates, or on step-grates, have proved to be efficient for burning small coal without smoke; but they require careful handling. Very much depends on the stoking where hand-firing is employed; although, inconsistently enough, the function of the human stoker is not held in much regard.

I have endeavoured concisely to bring before your notice matters of present interest to engineers and shipbuilders. We meet here for the interchange of ideas; and it only remains for me to acknowledge the readiness with which my brother engineers and others have supplied me with information, and to thank you for the attention with which you have listened to this address.

On the motion of Mr ROBERT DUNCAN, Vice-President, a cordial vote of thanks was awarded the President for his address.

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On Malleable-Iron Piping of Large Diameters for Conveying Water.

By Mr JOSEPH MOORE, of San Francisco.

The discussion of this paper, which was read on the 24th April, 1883, was resumed on the 23rd October, 1883, by

Mr JAMES M. GALE, who said the paper was one which deserved careful consideration at their hands, as it brought before them the application of materials to purposes not practised in this country. The wrought-iron pipes referred to in the paper were remarkable in respect of the great working strain that was put upon them. In the case of good wrought-iron, able to stand 20 lbs. pressure per ton, one-fourth was usually taken as the working strain, or 5 tons per square inch, and a further reduction of 30 per cent. was usually made for double-riveted lap joints, which brought them to 3½ tons as the strength of those wrought-iron structures. It was stated in the paper that the Spring Valley Water Works Co., which supplied San Francisco with water, had two lines of pipes of a total length of 40 miles, of 30 inches in diameter, which worked at a strain of 5½ tons instead of the 3½ tons above mentioned. In the paper Mr Moore told them that where little damage could be done by the pipes giving way, they run up the pressure to 8 tons on the square inch against the 3½ tons generally employed. These were strains that no engineer in this country would commit himself to. Then apparently they made no allowance for rusting. Indeed, these pipes did not appear to be strong enough to prevent them collapsing in the event of their being emptied of water; and large valves to admit the air had to be provided. These pipes were continuous riveted structures without any expansion joints, and though the

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range of the temperature of the water must be greater than in this country, it appears remarkable that no difficulty is experienced from the contraction and expansion, because it was known that in this country flange-jointed water pipes had been failures. Some of the pipes described were jointed with wrought-iron collars, which he considered to be a very excellent joint, and he had recently laid a 42-inch main under considerable pressure with that class of joint with great success. Then there was another remarkable thing connected with these wrought-iron pipes, and that was, the extraordinary rapidity with which they could be made. Mr Moore's firm had made 15 miles of a 30-inch main in less than six months, and had laid them at the rate of 1400 feet per day—certainly a most remarkable rate of speed. It was also noteworthy that when these pipes were superseded they cut them up and made them into others of smaller diameters. For instance, Mr Moore told them that a 19-inch pipe, which had conveyed water for nearly 15 years, had been remade into a 12-inch pipe. He thought these pipes were admirably suited for the requirements of the gold mines of California, as well as for a new country, where they were not expected to be of a very permanent nature. Indeed, for such purposes it would be almost impossible to use cast-iron pipes, by reason of the difficulty of carriage, from want of roads and distance of conveyance. Mr Moore on this point writes in the paper:—"It is not to be supposed that the author recommends wrought-iron pipes at all times where cast-iron can be got for nearly the same price, or where a permanent work is laid down; but it is believed that from the experience gained with it, its cheapness, and its durability when properly constructed, and the despatch with which it can be laid, there are many cases in which its use would pay. In temporary or preliminary works, there is no doubt it is worth serious consideration." He (Mr Gale) had no doubt that wrought-iron pipes might be profitably employed in permanent works where properly designed. The town of Kimberley, in South Africa, has 17 miles of a 14-inch wrought-iron pipe, the greatest thickness being $\frac{1}{4}$-inch, and the greatest pressure 500 feet, jointed with wrought-iron collars, and out of 6000 joints,

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only three of them were bad. The town, however, was 500 miles from the coast, and the carriage of materials up country cost £20 a ton—a sufficient reason for using wrought-iron. This kind of pipes was not suitable for street piping, although they had been so used in the Eastern States of America—but owing to the difficulty of making connections they were being abandoned. He thought they would not come into general use for usual sizes or for usual purposes, but for very high pressures of 300 or 400 feet, and for pipes over 4 feet diameter, he believed that if designed with a sufficient margin of strength, they would not be more expensive, and certainly they would be safer than cast-iron was. The usual mode of jointing cast-iron pipes was a source of great weakness, and also a source of danger. The collar joint was a very much safer joint, and with its use there was no chance of doing injury to the pipe, in setting up or driving up the joints; but cast-iron was altogether a very unreliable material. The last seven bursts of cast-iron pipes that had taken place in the Loch Katrine works, of the large sizes, between 48-inch and 36-inch, the tensile strain under which they gave way varied from 1374 lbs. to 300 lbs. on the square inch of section. This was a long way from the working strain usually put on cast-iron; while the pipes appeared to be all good castings, and burst from no apparent cause. In only one of the seven cases have the pipes failed at the socket. These pipes had all been in use for a long time—one for 23 years, and the others during periods ranging from 4¼ years to 7 years, and all had been tested in the usual way. He did not think that many engineers would undertake to lay a five feet cast-iron pipe under 300 feet of pressure, but there would be no difficulty in laying even a much larger pipe, to stand even greater pressure, if constructed of wrought-iron or steel. He did not see any difficulty in rolling a pipe so as to dispense with the longitudinal joints, and such a pipe might be jointed with collars, without rivetting at all.

The discussion was then adjourned till the next meeting.

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The discussion of this paper was resumed on the 28th November, 1883, by

M. NORMAND, Havre, who said that malleable-iron piping was used extensively in France for conveying gas and water, and had been found very serviceable. It had been introduced, and was yet almost exclusively supplied by the firm Chameroy & Co. It was considered that the coating with asphalte inside and outside had much to do with its efficiency. He had observed some of these pipes laid in a street in Paris near to the Opera House, of about 30 inches diameter and a quarter of an inch thick, but pipes of all diameters were now much used in France. He had seen some taken out of the ground without any appearance of being oxidised or decayed. His own opinion was that they were generally made rather light; but no doubt that had its advantage in cheapness. They also seemed remarkably durable, arising he had no doubt primarily from the coating they received from their flexibility, enabling them to yield without detriment in great measure to the movements of the soil and the pressure of passing traffic. Their superiority to cast-iron pipes was made apparent about a year ago in Paris, when an explosion of gas had taken place in one of the smaller streets, and four or five persons killed and several wounded. A cast-iron pipe had been broken or damaged by the passing weights or motion of the soil, and hence the escape of gas led to the explosion. He might mention that the wrought-iron pipes he had referred to as being laid near the Opera House were in close proximity to the surface of the street, and showed that the engineer had great faith in their power to withstand the effect of cartage. The Chameroy pipes were rivetted with five-eighths broad-headed rivets, in a single line, and about three inches apart from centre to centre. There was nothing like caulking, the tightness of the pipes lying in their coatings, inside and outside. The branches from the main pipes were made by a kind of collar tightened with a screw.

Mr MECHAN said his firm had a contract in hand for the Orita Mining Co., Savinila, South America, for seven miles of sheet-iron

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pipes, to convey water from a river to a gold mine. The work was being carried on by a Mr Welton, civil engineer, and was almost completed. The pipes were made in short lengths of 27 inches, as they had to be carried on mules' backs into the country, and put together by the natives in 13 feet lengths; and every 13 feet had a slip joint of 6 inches driven into each other tightly. The short lengths were made parallel, and expanded at one end so as to make a faucet joint. The rivets used were $\frac{5}{16}$, the pitch of holes in the circumference joints was 2 inches, the longitudinal joints were 1¼ inch and double rivetted. The run of water inside was put against the edge of spigot end of joint, and by running muddy or dirty water through the pipes at first it made them quite tight. The diameters were from 16 inches to 48 inches, the thickness of iron 10, 12, and 14 W.G. Mr Welton had done work of the same kind ten years ago, and said the pipes would last at least ten years. The pipes were laid on the surface of the ground, and when crossing such places as ditches or hollows, trees were cut down and the pipes laid across them. This contract was seven miles in length, and the total weight was about 200 tons.

M. NORMAND said that in Alsace water had been conveyed for 20 or 30 years to his knowledge a great distance for water power by the employment of wooden pipes. They were, in fact, merely pine boards bound together with hoops. Each of these pipes were made conical, so that the one would enter the other. They were found to be very economical, and lasted a long time.

Mr JOHN THOMSON said they were all pretty well aware that wrought-iron pipes had special purposes, and were only advantageously employed for such purposes. Of course, it was mainly a question of cost. In the circumstances described by Mr Moore in the paper, undoubtedly wrought-iron pipes were the right materials to employ. They were also properly used for the Kimberley Water Works in South Africa. There the cost of carriage had to be considered, as the cost of transit in these countries was about three times the original cost of the pipes in this country. Hence, in such circumstances, wrought-iron must be the right thing to employ; but

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they need not think that wrought-iron would come into general use in this country. If there was any material that might be expected to replace cast iron for pipes in the future, he thought it would be steel. With regard to the double-collar joint being used in Paris, he was of opinion that it had been abandoned there. His firm had a considerable contract for laying pipes in Paris at present, and they were all being joined with faucets, as in this country. They seemed to have discovered there that the double collar joint was a double expense, and had given it up. In the special joints referred to as made by Mr Gale, there was no perishable material used at all. The ends of the pipes were brought together and a ring expanded inside the pipe, and a steel collar put round the outside and the joint run in with lead.

The PRESIDENT did not know whether they had observed what Mr Gale had stated at the previous meeting in regard to the last seven bursts of cast-iron pipes in connection with the Loch Katrine Water Works—six of which had only been in use from 4¼ to 7 years. He would like to know whether Mr Thomson could give them any information as regarded these breakages, as only one of the original pipes, which had been laid 23 years, had given way. He would like to know whether the class of material was the same as that originally used, whether there might not be some cause arising from the subsidence of the soil, and whether wrought-iron pipes laid near the surface of the ground would not be as likely to give way as these cast-iron pipes?

Mr JOHN THOMSON said they could not admit they were using inferior metal; but it must be manifest that cast iron, like other metal, would give way under excessive fatigue. It was a difficult question to answer, however, why a pipe should give way sooner at one place than another. He thought that surface subsidence or superincumbent weights had something to do with it. They had very heavy weights now passing along the streets to the shipbuilding yards and engine shops, and he had no doubt these heavy castings passing along must put a great strain on the pipes below, and act injuriously upon them.

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Professor JAMES THOMSON thought they need not look for an explanation of these bursts of pipes to the steady pressure of the water, for he was of opinion that if the pipes could bear the water pressure at first, with a good margin of strength for safety, they ought to stand it for any length of time. The regulated pressure of water, he thought, did not fatigue them; but he thought the other causes that Mr Thomson had referred to—the subsidence of the ground and extra heavy weights passing over them—or any yielding in the joints, when very rigid, might be looked to as causing the fractures. He thought it was better not to entertain the notion that a structure that had been strong enough for a length of time had failed from fatigue.

The PRESIDENT said that as the discussion of the important question of the use of malleable-iron pipes did not seem to be finished, they would now adjourn it till another night.

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On the Stability of Ships at Launching.

By Mr J. H. Biles.

(SEE PLATES I., II., AND III.)

Received and Read 23rd October, 1883.

THE question of the stability of ships at launching has been brought prominently into notice in the last few months, by the deplorable accident to the "Daphne" at launching, and by the report of the Government Commissioner, Sir E. J. Reed, who conducted the official inquiry into the causes of this accident. Consequently, in responding to the invitation kindly given me by the Secretary of this Institution, to contribute a paper, I have considered that the subject chosen would be especially interesting at the present time, and is one upon which comparatively little has been written.

The subject naturally divides itself into two leading considerations—

1st, How much stability will a ship have when afloat?

2nd, How much stability should she have, in order to be safely and successfully launched?

The first may be termed a theoretical question, and is one which can only be settled by calculation and experiment; the second may be termed a practical question, and can only be settled approximately, by comparing the behaviour of ships at launching with their known qualities as to stability deduced by calculation and experiment.

The first is within the power and reach of any ship draughtsman, and is a branch of ship calculation which has been very much simplified by the invention of the integrator of M. Amsler.

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It is not my intention in this paper to attempt to explain the method of calculating the amount of stability which a ship has; but it may, however, be not considered out of place if an explanation is here given of the terms generally used in connection with the question of stability.

Stability may be defined to be the tendency of a vessel to remain in an upright or normal position. Consequently, for still water, the more difficult it is to heel a vessel, the more stability she may be said to have. This kind of stability is commonly called stiffness. When a ship lacks in stiffness, she is said to be tender.

For disturbed water, however, it often contributes to the stability of a vessel, as defined above, that she should not be very stiff, but that she should have the power of accommodating herself to the waves rather than of resisting too stubbornly their action upon her. If the conditions under which a ship is floating be remembered, the term "stability" may be quite safely used as a general term, the words "stiffness" and "tenderness" being retained for the more exclusive application to the case of ships at rest in still water—the term "steadiness" being the correlative for disturbed water.

We may fairly assume that the kind of stability which is required at launching is stiffness, for though the pressures which act on the bottom of a ship in the process of moving through the water, immediately after leaving the ways, are not the same as those which act upon her when she is at rest, yet they are not of the same kind as those which are acting on a vessel when she is among waves, and it is difficult to conceive that lack of stiffness can be of any advantage to a ship in launching. The question of stability at launching appears therefore to reduce itself to one of stiffness.

Reverting to the natural divisions of the consideration of the subject, the first now should read—

How much stiffness will a ship have when afloat?

Before this question can be satisfactorily answered we must determine a measure of a ship's stiffness.

The forces acting on a ship, when floating in still water, are the upward pressures of the water and the weight of the component

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parts of the structure. The effect of the former may be considered as that of a single force equal to the sum of these pressures, and acting upwards through the centre of gravity of the volume of the water displaced by the ship, this centre being better known as the centre of buoyancy. The effect of the latter set of forces (the weights of the component parts of the structure) may be considered as that of a single force equal to the sum of these weights, and as acting downwards through the centre of gravity of these weights. When a ship in stable equilibrium is floating at rest in still water, these two points are in the same vertical line as in Fig. 1, Plate I. When a ship is heeled over as in Fig. 2, Plate I., the centre of gravity of the weights of the ship does not move, but the centre of buoyancy, B, moves out towards the submerged side, and the farther out the latter moves, the more tendency will the water pressure have to cause the ship to return to her upright or normal position; in other words, the stiffer will the ship be. To be more precise, the farther the vertical through the centre of buoyancy moves away from the vertical through the centre of gravity of the weights at any given angle of heel, the stiffer the ship is. Hence, this distance is a measure of the stiffness of the ship, and, having found it, we can say that, for the given angle, we know how much stability the ship has.

In a paper which I had the honour of reading, in March, 1882, before the Institution of Naval Architects, in London, will be found a full explanation of the method of determining this distance for any angle of heel.

Suppose that we have found the values of the distance G Z, between the two verticals referred to in Fig. 2, Plate I., for several different angles of inclination, say 10, 20, 30, 40, 50, &c., and (referring to Fig. 3, Plate I.), set off along a line distances representing 10°, 20°, 30°, 40°, 50°, &c., and at these points set up the values G Z, of the distances between the weight and buoyancy verticals determined for these angles of heel. The curve drawn through the points Z, Z, Z, so set off, is called the curve of stability, or, if we wish to particularise the kind of stability, we may call it

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the curve of stiffness. We are in the habit of speaking of the stiffness of a ship in reference to small angles of inclination, near the upright position, and therefore the part of the curve up to 10 degrees represents the stiffness we usually speak of; the greater the slope of this part of the curve—that is, the more rapid the increase of G Z, the greater is the stiffness of the ship as ordinarily understood.

Referring to Fig. 2, Plate I., if we produce the vertical through the centre of buoyancy, it will cut the original vertical in the point M. For a given angle, the higher this point is, the greater must be the distance G Z between the buoyancy and weight verticals, and therefore the distance G M measures the distance quite as well as the distance G Z, for a given angle. For small angles it is found that this point M does not vary much, and therefore if we know its position we usually have a measure of the stiffness of a ship for small inclinations. This point is called the *metacentre*, and its distance G M, from the centre of gravity is called the *metacentric height*. Its position is more easily calculated than that of the point B (Fig. 3, Plate I.) Hence it has been much more frequently determined for ships than the curve of stability (Fig. 3, Plate I.) This metacentric height G M, usually measures the stiffness of a ship up to 10° quite accurately enough for all practical purposes, and, until about 15 years ago it was thought that if a ship had a good metacentric height her stability in all conditions was undoubted. This fallacy was disproved by some of the eminent Admiralty naval architects, who showed that it was also necessary to have a fair amount of freeboard in order to have stability at large angles of heel. It was not, however, until the "Captain" turned over at sea that this fact was thoroughly realised. This unfortunate ship had a sufficient metacentric height, but a very low freeboard, and consequently she had a sufficiently steep curve of stability or stiffness at the small angles, but on account of the want of freeboard, the stability rapidly diminished at the large angles of heel. After this disaster it became generally recognised that in order to have sufficient stability it was necessary to have considerable freeboard, and that if such were

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provided the metacentric height might be small. The careening at launching of a large steamer led to the discovery that this also was a fallacy, and that even with a high side it did not necessarily follow that a ship, in the light condition in which ships are usually launched, had sufficient stability at large angles of heel. This fact, however, was not fully realised until it was pointed out at the Commissioners' Inquiry into the cause of the upsetting of the "Daphne."

It becomes apparent then that our only safe guide is the complete investigation of the stability of a ship at angles considerably beyond those to which the metacentric height is a fair measure of the stiffness of a ship.

For a complete solution of the first division of the subject it is necessary to know—

1st, The position of the centre of gravity of the weights of the component parts of the structure.

2nd, The amount of stability of a ship at the launching draft up to, at least, 60° of inclination.

The first of these can most readily be determined by inclining ships at their launching draft. A mass of data can thus be readily and easily accumulated from which the position of the centre of gravity of a similar ship can be readily determined, proper allowance being made for any small differences in the structure.

The second can be determined by Amsler's integrator, as already explained.

Having come to the conclusion that the only safe guide in these matters is the complete curve of stability, and in order to have as much information on this subject as possible, Messrs Thomson have had the curves of stability of a large number of their ships calculated, and I have their permission to publish a few which may be interesting to the members of this Institution.

Fig. 4, Plate II., represents the curves of two steamers which were launched successfully. The centre of gravity of each was found by experiment immediately after launch.

A is a vessel 275 feet long, 35 feet beam, 21½ feet deep, moulded.

B is a vessel 430 feet long, 43 feet beam, 35 feet deep, moulded.

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The first was launched and did not give the slightest sign of heeling; the second took an inclination of about 20° at launching, but came to rest in an upright position after some five or six rolls. In the latter case there was nothing in the ship which could shift, so that the condition usually assumed in stability calculations was the actual condition of the ship.

These two cases are interesting as showing in one case how much stability a vessel may have when launched, and in the other how little is actually necessary when proper precautions are taken and circumstances are favourable.

Fig. 5, Plate II., gives the curves of stability of several types of ships all reduced to 25 feet beam and 6·665 feet draft with a centre of gravity 11 feet above the keel. The following are the particulars of the different types arranged in their order of fineness as measured by their block co-efficients:—

	TYPES.	Block Co-efficients.	Length.	Breadth.	Depth Moulded.	Draft.	Metacentric Curve.
Fig. 5.	C - - -	·42	175	25	21·52	6·665	·1
	D - - -	·473	„	„	21·73	„	–·31
	E - - -	·511	„	„	20·62	„	·42
	F - - -	·524	„	„	21·53	„	·25
	G - - -	·539	„	„	20·17	„	1·04
	A - - -	·548	„	„	21·5	„	·66
	B - - -	·57	„	„	20·204	„	·74
Fig. 7.	(1) Type A, - -	·548	175	25	21·5	6·665	·66
	(2) Type C, - -	·42	175	25	21·52	6·665	+·1
	(3) Type C, Beam and Length increased to make Displacement same as A,	·42	200	28·6	21·52	6·665	2·15
	(4) Type C, Length and Draft increased to make Displacement same as A,	·45	200	25	21·52	7·1	–·1

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The finer types appear at a disadvantage compared with the fuller ones. For the same moulded depth, however, a fine ship will usually have a greater draft than a full one at launching.

Fig. 7, Plate III., gives the curves of stability of the fullest and finest of these types, modified as in (3) by increasing beam and length of the finest (C), to give it the same displacement as the fullest (A). (4) in Fig. 7 is the curve obtained by increasing the draft and length, instead of the beam and length, as in (3).

These three sets of curves are interesting as showing the effect, 1st, *of form* on the same dimensions; 2nd, *of draft* on the same breadth and displacement in different forms; 3rd, the effect of beam upon the same length and draft but different forms.

We come next to the consideration of the second part of this question,

How much stability should a ship have, in order to be safely and successfully launched?

This is a question which is difficult of exact solution. So many disturbing forces are at work, in the short interval which elapses between the time at which a ship leaves the ways and that at which she is safely floating at rest, that an *à priori* solution is impossible. Until the ship comes to rest the conditions which hold are not those which are assumed as the basis of the curve of stability. The principal differences are due to the following:—

1st. The disturbance which the ship herself creates by her sudden intrusion into the comparatively small quantity of water in the vicinity of most shipbuilding yards. In narrow rivers, where the water is shallow, waves are produced which may be reflected from the opposite bank on to the ship. Whether they are or not must depend upon the angle at which the ship is launched, her form, speed of entry, and the breadth of the river, but it is not difficult to conceive of a wave being reflected back on to a ship so as to cause her to heel considerably. If there is any considerable tide in a shallow river the sudden introduction of a large ship, stretching in many cases half way across the river, must have the

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effect of heaping up the water on one side, and causing a depression on the other, thus tending to give the ship an excess of buoyancy on the tide side of the vessel. The evidence in favour of the existence of two currents flowing in opposite directions has not received much support from observers of tidal action, and it is somewhat difficult to imagine that the currents would flow in the directions they are stated to flow; for the up current would almost certainly be salter and therefore denser than the down current, and would consequently be an underneath one. The opposite view is the one which was taken by some eminent witnesses in the recent Inquiry.

2nd. The pressure of the wind upon the side of the vessel. In the case of the vessel B, the total wind pressure upon her side is 5·4 tons for every pound per square foot of wind pressure, and its moment is about 100 foot tons. One pound per square foot of wind pressure is capable of heeling the ship about 3°.

3rd. The unequal effect of the buoyancy of the bilge ways caused by the release of a larger quantity on one side than on the other. If the whole of one side be assumed to be released but only the half of the other, the moment tending to upset the ship B would be 50 foot tons, and the angle of heel caused by this would be about 1½°. This cause is not a very powerful one.

4th. The shifting of loose weights on board the ship if she is heeled by any of the above causes. This is most likely to be dangerous to a ship, as it is a cause of disturbance of a permanent nature, and not like the preceding ones, which may all act only for a short time. In the ship B if a weight of 40 tons (not an unusual quantity for a large ship to have loose upon her deck) be assumed to slide nearly halfway across the ship after she has heeled to 20°, the effect will be represented by the alteration in the curve of stability to the part coloured in red (Fig. 4, Plate II.) This represents a considerable reduction of her margin of stability. The curve is also shown reduced on the same assumption for the ship A. The percentage of reduction is not so large in this case, though on account of the smaller size of the ship the actual reduction is very much larger.

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5th. The heeling action due to the checking of one drag chain before the other. If a ship is continuing to move in the line of the ways and she is checked on one side only there will be a tendency to slew the ship, and this must necessarily cause her to heel as the resistance to slewing is acting under the water at about half draft, whereas the force producing the slewing is acting considerably above the water. If before the chains check the ship the tide has slewed her, this action may be aggravated.

It is very difficult to calculate the actual heeling effect of the drag of the chains, but from observation of the distance the anchors are dragged through the ground, the time taken to drag them, and the speed of the ship, some idea of this force may be obtained. I regret that the information which I have at present on this point is not sufficient to enable me to give any definite opinion, but from observation taken at launches, the heeling of a ship by drag chains does not seem to be very frequent.

There are probably other causes which have not been mentioned, but as some already mentioned cannot be measured, it is very difficult, if not quite impossible, to say, without experience, how much stability a ship *ought to have at launching.*

The best information which we have on this point is represented in curve B, Fig. 4, Plate II. This ship was launched successfully with ·66 ft. of metacentric height, but she took a considerable roll, and had anything been loose in her it is probable that she would not have returned. Later on we launched a ship with the same metacentric height, but there happened to be sufficient loose material about, and this, combined with other causes, was sufficiently potent to prevent the ship from returning to the upright. Hence, it is fair to assume that ·66 feet is too little, but when proper precautions are taken and circumstances are favourable, a ship may be successfully launched with this metacentric height.

As a working minimum, however, if *the curve of stability does not show any great peculiarities*, but if it increases gradually between 0 and 50 degs., it seems that one foot may be taken. I am of opinion that at least this amount should be given in all ships, whether large

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or small, but that in small fine ships a curve of stability should be calculated.

Fig. 6, Plate III., shows the ships in Fig. 5, on the assumption that each has a metacentric height of 1 foot.

A careful study of the curves given will, it is hoped, be of interest and value to the members of this Institution; and, if in the least degree I have added to the available stock of knowledge in this question, my object in reading this paper will have been attained.

I venture to express a hope that the experience of other shipbuilders besides Messrs J. & G. Thomson may be laid before this Institution at some time, in order that a correct solution shall be given to the general question:—How much stability does a ship require to perform an intended purpose?

The discussion of this paper was commenced on the 20th November, 1883.

Mr LAURENCE HILL said he was of opinion that it would be wrong for a shipbuilder to calculate the amount of stability required at launching too closely. When a ship was being launched the weights should be put low down so as to make assurance doubly sure. If the builder was not certain that the vessel were perfectly safe at a very great angle, let him put in pig-iron so as to bring the centre of gravity well down, which expenditure would be repaid by the feeling of security it would yield him. No doubt the place and time and weather modified things very much in regard to launching vessels. In summer, in a straight out place like what he had at Port-Glasgow, it required very little care compared with that necessary in a narrow channel; but it was undoubtedly the safest plan to have the centre of gravity well down. Notwithstanding all that had been said, and would be said yet, regarding curves of stability and metacentre—all very necessary in their own way, and which apparently had led the public to expect that every vessel launched ought to be capable of righting itself under any circumstances at launching, all builders knew this would not be the case, as vessels had to be built in accordance with the requirements of trade. No doubt it was quite

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possible to launch a vessel not only without ballast, but which would right herself when properly loaded, and yet that vessel might be upset by a stevedore through careless loading. That gave rise to another very important question—How much knowledge had stevedores or owners on this matter of loading? The builder might calculate the curve of stability correctly, and have everything quite safe for the launching and sailing of a vessel, but if the stevedore was allowed by his loading to shift the centre of gravity, and so alter a very important factor, every time the vessel was filled the ship's safety was imperilled. He was of opinion that a good many more vessels had been lost on that account than the public were aware of.

Mr Wm. Macmillan said that in the paper Mr Biles had brought before them a subject of considerable interest at the present time, showing the dangers which attended the launching of ships, where proper forethought had not been exercised. In his valuable paper, Mr Biles stated, that the only safe guide in these matters was a curve of stability. With this he did not agree, for if they investigated the matter a little further, they would find it was unnecessary previous to launching, for the purpose of securing safety. In launching what was required was stiffness; not range of stability. Hence, all that was necessary was to calculate the height of metacentre and centre of gravity. If this showed an insufficient interval to secure stiffness; it ought to be increased by placing ballast on board. In addition, weights ought not to be stowed to one side of the vessel; no loose materials should be lying about; and the ports ought all to be closed. Mr Biles referred to a ship which was launched with ·66 feet of metacentric height. This vessel took a considerable roll, and had anything been loose in her, it was probable she would not have returned. Later, a ship had been launched with the same metacentric height; but there happened to be sufficient loose material about, and that combined with other causes, was sufficiently potent to prevent the ship returning to the upright. In all likelihood, the chief cause which prevented this vessel from returning to the upright was the loose materials. It would be interesting if Mr Biles could

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state the weight of loose material on board the vessel referred to; and what angle this weight moved through a given distance would incline her to. There had been so much made of this curve lately, that one would think it possessed some magical influence over ships during launching, loading, or sailing. Supposing a curve of stability had been calculated for the ill-fated "Eurydice," which, within sight of home, capsized, with a lamentable loss of life, was it at all likely that the officer in charge of this vessel would, on account of the gusty weather, have consulted his curve of stability, and from it come to the conclusion that the ports should be closed? He thought not. After long experience he had come to the conclusion that ship-builders must be guided by practical common sense, as well as theory. If they looked back over the record of ships launched on the Clyde during the memory of any member of this Institution, or perhaps further, excluding the "Hammonia" and "Daphne," they would find that the failures had been infinitesimal, and in every case loose materials had played a very important part in causing disaster. Therefore he had no hesitation in saying that if they bestowed in the future the attention and care which had ruled in the past, in guarding against loose materials, and making sure of a sufficient metacentric height, success would be the result. It was only in 1867 that the first curve of stability had been calculated, and as the ships had been launched successfully previously without it, so he was satisfied they would in the future. From what he had said, they must not run away with the idea that he considered a curve of stability of no value; that was far from his opinion, but he did think that for the purpose of launching, it was unnecessary.

Mr F. P. PURVIS referred, first, to a remark made by Mr Hill—that it was better to err on the safe side—and remarked that that gentleman had not specified to what extent he would suggest they ought to go to the safe side, or what they ought to consider was a margin of safety. Mr Biles had taken a metacentric height of eight inches, or, to be on the safe side, of one foot. He thought that was definite, and they might discuss it. While agreeing in a general way that that was sufficient, and that it was necessary, he would remind

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them that they ought not only to expect eight inches of metacentric height, but ought to see that they got it. If they turned to the evidence in the case of the "Daphne," they would find that while her launching draft was estimated by Mr Elgar at seven feet, it was really eight feet five inches. Her centre of gravity was rightly estimated, but the excess of draft was such that the eight inches of metacentric height which Mr Elgar estimated for her, and which she should have had, according to Mr Biles, had turned out to be less than four inches. That was a large diminution. Now, a parallel case may happen any day, and provision must be made for it. It was well to remember that the metacentre at launching draft varies considerably with a small increase of draft. For instance, an increase of draft of six inches will often bring the metacentre down six inches; it may be more or less, according to the lightness of the draft. So that if by calculation a ship be estimated to have ten feet of draft and one foot of metacentric height, but, being heavier, proved to have a draft of ten feet six inches, the metacentric height will from this cause be reduced to six inches instead of one foot. That should be kept carefully in mind. So much with regard to this point. There was one point, however, to which Mr Biles had not called attention, and that was, that all the ports and the openings should be closed and loose material should be carefully secured. Messrs William Denny & Bros. had a general order that particular attention should be paid to have no loose materials about the decks, and to have every opening carefully secured before the ship was put into the water. With respect to the diagrams illustrating the paper: They showed, in a highly interesting manner, that there are differences to be obtained by fineness of form, although the vessels be of the same breadth and depth, and have approximately the same position of the centre of gravity. Messrs Wm. Denny & Bros. had been trying to get at some approximate means of determining a curve of stability without the labour of calculating it accurately. They had aimed at doing it for any breadth, any draught, and any freeboard. So far as they had gone they had paid no attention to fineness of form. He had no doubt that Mr Biles' diagrams would

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be of great use to them. For instance, in estimating for a ship which had a different metacentre from the ship they were starting from, due to the fulness or fineness of its form, they could make two assumptions. They knew—he would assume—the centre of gravity of the ship for which they were making calculations. They could refer back and assume it in the same relative position in the depth in the ship from which they had started; or they could assume it not in the same position relative to the depth, but at the same distance from the metacentre. They would get two curves, the one of which—viz., that in which the metacentric height was correctly taken—would certainly be most nearly correct for small angles; while the other—viz., that in which the centre of gravity was taken at its correct position in the depth of the ship—would prove most nearly correct for large angles; the proximation of the curves A and C at 90 degs. might be taken as a proof of this statement. Perhaps some other time he might be able to say more about this approximate working. For himself, he begged to thank Mr Biles for the diagrams he had laid before them.

M. NORMAND said he was sure they were all much indebted to Mr Biles for the great pains he had taken in bringing the paper before them, and for the light he had thrown on this very important subject. He believed it had been a constant subject of consideration for all shipbuilders and engineers for the last three or four months. He agreed with the necessity of giving a good margin of stability in the launching of a ship. He thought Mr Biles had forgotten to point out the great importance of ascertaining, previous to launching, that all the weights were situated in the middle line of the ship, including the permanent weights, such as the engines and boilers. He considered that the almost exclusive research for the remedy by infinitesimal calculations of stability up to the most inconceivable angles of inclination, such as 90 degrees or more, was at the least injudicious, for two capital reasons:—1, A ship should never be in a condition to incline at launching, therefore the case could not be how she could return to the upright position, as she should never have been allowed to depart from it; 2, It had been once more

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demonstrated that the only circumstances in which a ship might be met with a reducing or vanishing stability at great inclination was in the case of a small proportion of freeboard, which evidently could not happen with a ship in launching condition. In conclusion, it was evident that not only the best, but the only element of security, in all doubtful curves, was to be sought in a liberal employment of ballast; there could not be any objection to securing thus the most ample margin of stability—that is, something like two feet of net metacentric height. Nobody could object that in this employment of ballast on the day of launching any deduction would result in the carrying capacity of the ship.

Mr. G. L. WATSON said that the curves given in Mr. Biles' paper were extremely interesting, but to his mind they themselves showed that the getting them out for any particular vessel was not of much practical use. Any one who was acquainted with ships and their proportions would see at once, on looking at a ship or her drawings, if she was at all likely to be ticklish; and in preparing a ship for launching, if she did look tender, would make more or less exact calculation of her centre of gravity and her metacentre, and if they were found to be dangerously near each other, it would cost less, both in time and money, to put in ballast than to set about calculating her curve of stability. Of course this was a very unscientific way of looking at the matter, but ships were built in the first place to pay, and he could see nothing in these curves to make them distrust the metacentric estimate of stability for launching purposes. A curve of stability would not help to keep a boat upright, but ballast would. He agreed with what M. Normand had said with regard to having the centre of gravity in the central axis of the vessel at launching as at all other times.

Mr. GEORGE A. AGNEW agreed with what Mr Biles had said with regard to a curve of stability being necessary for small fine vessels, but he went further and said it was necessary also for large fine ships. As to the proper amount of stability in a given case, Mr Biles gave an instance in the curve B, Fig. 4, Plate II., where the metacentric height was ·66. That ship inclined some 20 degs. in launching, and from this case Mr Biles considered the minimum

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metacentric height should not be less than one foot. The firm with which he was connected (Messrs R. Napier & Sons), made a curve of stability for each ship before launching. The curve which he held in his hand—one of the first, if not the first made before launching, the curve being slightly corrected after the centre of gravity had been checked by the heeling experiment—referred to a vessel built by that firm, in which the actual metacentric height was 2 feet 2 inches, and that ship inclined in launching nearly 15 degs. This was an extremely fine vessel, her co-efficient of displacement being ·46. For comparison he had calculated the curve for this vessel (see Fig. 5A, Plate IIA.), with an assumed metacentric height of 15 inches, or nearly twice Mr Biles' ship, which, strange to say, the curve exactly coincided with his curve B, up to about 25 degs., and in this case showed that 1 foot 3 inches was not sufficient, after making the same allowance as Mr Biles had done he had a metacentric height of nearly 2 feet as a minimum for launching. The actual metacentric height, as already stated, was 2 feet 2 inches, and the stability was nothing more than was necessary; and therefore he agreed with the necessity for calculating launching curves of stability. As to the keeping of weights in the centre of vessels, the firm with which he was connected was in the habit of paying particular attention to this matter, arranging the boilers a little to the one side of the centre of the line, and all the other weights in like manner, so as to compensate for excesses of weight that might be either on the one side or the other, and so keeping the centre of gravity of the whole coals or machinery in the centre of the ship.

Mr. ROBERT T. NAPIER said he did not think that Mr Biles had thrown light on his subject by his definition of the terms "stiffness" and "steadiness." As at present understood a "stiff" ship was one with high initial stability; she had a quick period of roll, and when among waves tended to keep the position of equilibrium, that is, her deck parallel to the wave surface. A "steady" ship, on the contrary, had low initial stability and a long period of roll; she made less effort to retain the position of equilibrium when among waves, and had consequently easier motion than the other. With respect to curves of stability, the reason why they had not been more used

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was the tediousness of the calculation involved. The weight of hull can be got with ease and certainty to within two per cent., and the position of the centre of buoyancy with mathematical accuracy, but the determination of the position of centre of gravity was a heavy and far from satisfactory job. As many ships have of late had the position of their respective centres of gravity determined by experiment, it would be a most valuable addition to the Transactions if the builders would allow the results to be published. With a sufficient number of examples before them the position of the centre of gravity of a given ship could be fixed with a fair degree of accuracy. For the launch of an ordinary vessel it would usually be sufficient to try the angle at which stability vanished, that is, when the point G in Fig. 2 coincided with the point Z. This could easily be found by trial and error assuming, which was near enough for the purpose, that the height of the centre of figure of the mean immersed section of the vessel was the same as that of the centre of buoyancy. If a ship could be prevented from rolling to this angle she was, of course, safe.

Professor JAMES THOMSON, in referring to the early passages in the paper where distinctions were indicated among different kinds of stability of ships for still and for disturbed water, and conditions were stated as those which would tend to bring about the character of steadiness for disturbed water, said he thought his remarks on this matter would be greatly in agreement or quite consistent with those of the previous speaker, Mr Napier. He agreed with the author's statement to the effect that, for disturbed water, it often contributes to the stability of a vessel that she should not be very stiff (the name stability being understood here in the sense of steadiness with avoidance of rolling through great angles of declination from the upright position; and stiffness being understood as the character which is manifested by great resistance to heeling in still water). When, however, the author goes on in the same sentence, after the recommendation that she should not be very stiff, to say, but that she should have the power of accommodating herself to the waves rather than of resisting too stubbornly their action upon her, he (Professor Thomson) thought the author was falling into one or two oversights or casual fallacies. The property

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of being very stiff (that is stiff against being heeled in still water) would not tend adversely to the ship's power or readiness to accommodate herself to the waves, and would not constitute stiffness in her for resisting stubbornly their action upon her. On the contrary, that kind of stiffness would rather involve a tendency for the ship very firmly and briskly to accommodate herself to the waves in spite of the resistance of the inertia of the body of the ship and masts and rigging to the reception of rotation, or change of rotation, round a longitudinal axis; and so through that kind of stiffness the ship, and more especially the rigging, would have an unsteady and uneasy quality in their motions. Rather it is wanted with a view to attainment of a good condition of easiness and steadiness that the ship should have sufficient moment of inertia round a longitudinal axis, coupled with a small enough stiffness as to insisting on accommodating itself to the closely surrounding water surface, including a small enough stiffness as to insisting on turning with the wave surfaces, to give her a very long period of oscillation, and especially so long a period that the successive impulses to oscillate shall not have much of a cumulative tendency or cumulative resulting effect. He believed much benefit would result from such investigations as those Mr Biles had brought before the Institution. It was very important to have methods in readiness for ascertaining very fully the capabilities of a ship as to its future behaviour, by calculations beforehand. It would then become a matter for practical judgment to decide, in particular cases, how much of such scientific prospective calculations might be desirable and profitable, not merely in a money point of view but as a guarantee towards safety against risks to life and property. In reference to the remarks made in the paper respecting an alleged under current flowing down a river while the tide is flowing up, near the time of high water, Professor Thomson thought it extremely unlikely that any such current as had been alluded to could have any important influence on the launching of a ship. Still he was aware of a physical principle in hydrodynamics, a principle which he had witnessed exemplified in fact, under which the occurrence of a reverse under current in such circumstances was explained, and was shown to be a necessary occurrence at a certain stage of the tidal

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flow. Considering as he did that this hydrodynamic principle, though interesting and important in itself and as throwing light on some conditions of tidal currents, was unimportant in reference to stability of ships at launching, he would not occupy the time of the meeting by bringing it under consideration on that occasion. He suggested, however, that he might perhaps offer a brief paper on the subject for some future meeting of the Institution.

Mr JOHN BROWN said that, from the figures on the diagram before them, he observed that the draft was given there to 1-200th part of a foot. He asked if that was practicable? Could they tell the draft of a vessel to the 200th part of a foot? Then he noticed that some of the metacentres were given to the 1-10th part of a foot. Now, a gentleman had mentioned that evening a case where a metacentric height was four inches out, or nearly 4-10ths of a foot; another gentleman, one in which the calculated height of the centre of gravity was one inch out, so that the metacentric height of 1-10th of a foot would be entirely swallowed up. He asked whether these figures were reliable, or what was the range of error in those curves?

The PRESIDENT replied that Mr Biles gave them as correct, and that there was no error in them.

On the motion of Mr ROBERT DUNCAN the discussion was adjourned till next general meeting.

The discussion of this paper was resumed on the 18th December, 1883, by

Mr GEORGE THOMSON, who said he wished to make a few observations on the question of stability at launching. Having always looked on statical stability from the same basis as that of longitudinal trim, he noticed that on launching a vessel the first thing was the action of longitudinal stability. The ship went down on the angle of the standing ways till she came to her displacement, the centre of buoyancy fell aft beyond gravity, and the excess of buoyancy made her lift, dip by the bow, and but for the drag-chains the momentum gained in sliding down the ways would be spent in the space beyond the launch. Of course in a narrow

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river like the Clyde the ship was pulled up by the drags, and waves were generated by the reaction of the displaced water on the river banks, and probably oscillation might be increased by one side of the launching ways and making up, clearing away on the one side before the other. This force, to his mind, appeared very trivial, for at launching he had never seen a ship oscillate more than about 5 degrees, and judging from appearance nothing like 10 degrees. Mr Biles gave a metacentric height of 7 inches for one vessel. This was far too small; though a ship would sit up with 7 inches of true metacentric height, and also with 3 inches of true metacentric height without a breath of wind in still water, that would not do for launching. It was difficult to put one's thumb on the exact metacentre of a real ship, and more so to put one's thumb on the centre of gravity before launching; therefore, a margin should be made on the calculations, and instead of 7 inches of metacentric height, he would advise giving her 7 inches of righting lever at an angle of 10 degrees. A ship could not list (with her centre of gravity below the vertex of the centre of buoyancy) with her centre of gravity on the weather side without a force equivalent, any more than they could expect a ship to go by the stern with her centre of gravity before the centre of buoyancy. It was then a matter of indifference where the altered loci of metacentre went; so long as gravity keeps the weather side in the middle line of the ship of the altered buoyancy vertical, she must come back to the upright. He had found that by using the ordinary box rule (a very simple formulæ)

$$\frac{\frac{2}{3} \text{ cube of } \frac{1}{2} \text{ breadth}}{\text{moulded breadth} \times \text{moulded draft}}$$

—that is, cube the half-breadth of the midsection at the respective moulded drafts, two-thirds of this divided by breadth, multiplied into depth, would give for our present type of ships the height of the metacentre above centre of buoyancy within a couple of inches. He did not mean to say that this would suit V-shaped ships. On making experiments on models, he put more faith in this than taking the moment of inertia of the water-line plane, divided by the displacement in cubic feet at the light drafts. With regard to the integrator mentioned by Mr

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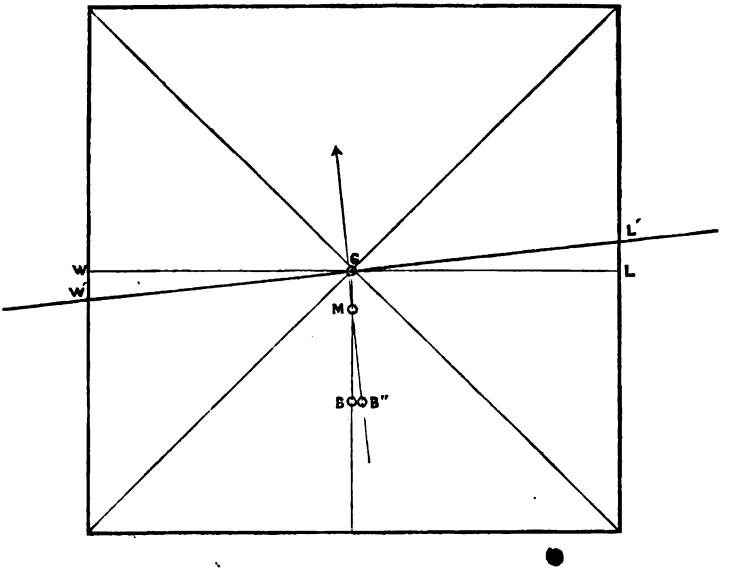
Biles, though he had seen it in Messrs Denny's in Dumbarton, he had not used it. No doubt it was a very good instrument. However, the ordinary planimeter does all that is wanted remarkably well, and by making a few displacement curves at different angles of heel, the weight of the hull could be sliced off, and the depth of the centre of buoyancy got without trial and error. By making another curve in an opposite direction, the distance across of the centre is got without troubling about moments for leverage. Meantime he intended to teach the young men in the shipbuilding yard drawing office at Meadowside this method, as it developed at a glance the faults and beauties of a ship's stability at the various angles.

Sir E. J. REED kindly contributed the following remarks upon the "metacentre" of a ship to the discussion, which the Secretary read: —There have been scientific persons who have contended that this word "metacentre" always has, or should have relation to a position of equilibrium, being that point in the vertical axis of the body (when in equilibrium) which limits the height to which the centre of gravity can be raised consistently with the equilibrium remaining stable. If (as in Fig. A for example) G be situated above M, then



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stable equilibrium does not and cannot exist. This limiting height of G is ascertained by giving the body a very slight inclination from a position of equilibrium, as we have done in Fig. A, and finding the point M at which the vertical through the new centre of buoyancy cuts the vertical through the previous centre of buoyancy. All the time this view of the metacentre is borne in mind, and the word is associated with a position of statical equilibrium, it is perfectly correct and safe to speak of the *height* of the metacentre as ruling the stability. But the word metacentre has been employed with other meanings. It has sometimes been qualified by an adjective, as "shifting metacentre"—a term first employed probably in a work on "Shipbuilding," edited by the late Professor Rankine—signifying the intersection of an axis of equilibrium (or a vertical through the centre of buoyancy in a position of equilibrium), with a vertical through the new centre of buoyancy when the angle of inclination is not very small, but a considerable angle such as a ship may be supposed to incline to. The word metacentre has been further applied to the ultimate intersection of two verticals through adjacent centres of buoyancy, even when this intersection lies apart from an axis of equilibrium, provided the angles of inclination of the body are somewhat near a position of equilibrium. It is in this sense that Bouguer wrote the chapter of his *Traité du Navire* headed "On the metacentres, and on the curved line which these points form when the ship is inclined," in which chapter he designated the locus of these so-called metacentres the *metacentrique*, and discussed the supposed (but erroneously supposed) effect upon stability of the rising or falling of this curve above or below a metacentre situated in the axis of equilibrium. Lastly, the word metacentre has sometimes been employed in a more extended sense still, being taken to signify the intersection of any two verticals through adjacent centres of buoyancy taken very close together, without any reference to a position of equilibrium, or to an axis of equilibrium.

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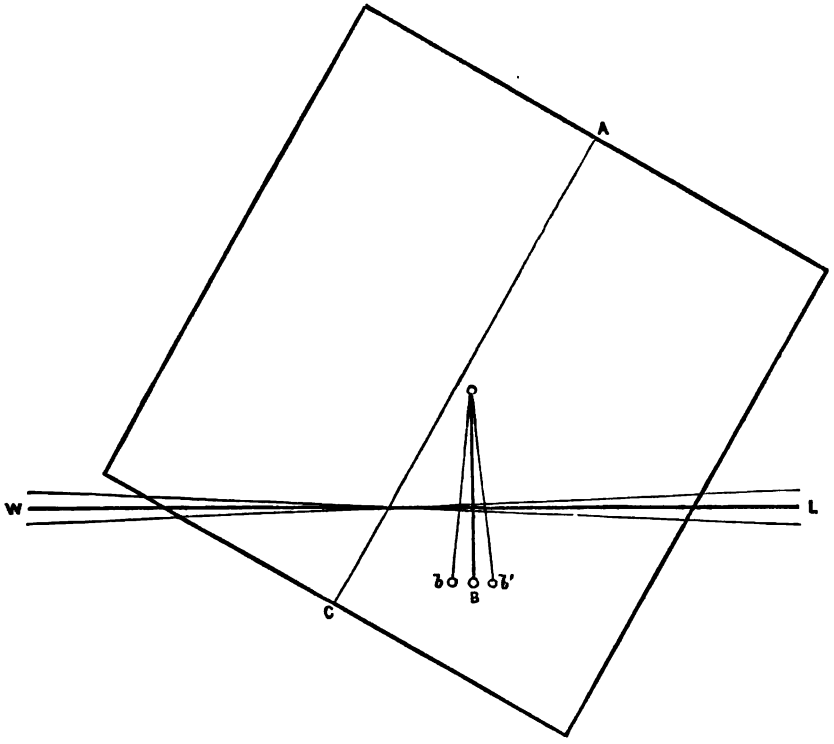

In Fig. B this is illustrated. A floating prism of square section is there shown with the axis A C inclined at an angle of about 30 degrees from the upright. B is its centre of buoyancy, when W L is its water-line. By giving the body a very slight inclination either way from this position it will have a new centre of buoyancy given to it. If we incline it one way *b* will indicate this; if we incline the other way *b'* will indicate it; and for each of these positions there will be a new line of upward action of the buoyancy. These lines of action, together with that through B, will all meet or intersect in one point M, and this point will be the so-called metacentre—in the most extended sense of the term—at 30 degrees of inclination. The term metacentric (*metacentrique*) has come to be regarded in a

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similarly extended sense, and taken to signify the locus of all such metacentres as has been last defined.

It cannot be doubted that grave inconveniences, and in imaginable cases even dangers may result from giving these several extensions to the meaning of the word "metacentre." In the case of the "shifting metacentre" so-called, it is certainly no metacentre at all in the true and original and most accepted sense of the term, for it is not a limiting position for the height of the body's centre of gravity, in the sense of separating stable equilibrium from unstable. The centre of gravity may conceivably lie between the true metacentre and one of the shifting metacentres so called, and then the floating body will have to pass through a position of equilibrium while inclining from the one position to the other, and one of these two positions must be one of unstable equilibrium. This would have involved but little danger or disadvantage in former times; but with modern steamships, in which the metacentric stability is often exceedingly small, and the metacentre is not unfrequently found below the centre of gravity, it is most undesirable to sanction the continued employment of misleading terms. I therefore intend to discard altogether this term "shifting metacentre," which, as we have seen, is no metacentre at all, and substitute for it the term "False Metacentre." Wherever the "*false metacentre*" is hereafter spoken of by me, it will signify the point at which a vertical through a centre of buoyancy cuts the axis (or given axis) of equilibrium at any considerable angle; or, in other words, it will be the point which Rankine intended to define when he said, "When the position of the point M (the metacentre) varies for different angles of heel, its proper designation is the "shifting metacentre." I think its proper designation is *False Metacentre*, and shall thus designate it hereafter. In the case of the extension which Bouguer, as we have seen, gave to the meaning of the word metacentre, he was himself led thereby into the error which Atwood subsequently corrected. It was a complete mistake to suppose, as Bouguer did, that the mere rise or fall of the *metacentric* necessarily indicated increase or decrease of righting force. I propose, there-

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fore, notwithstanding my admiration of Bouguer and my great respect for his work, to discard both his extended signification of the word *metacentre*, and his employment of the word *metacentric*. The necessity for having one specific and well-defined signification of the word "metacentric" is so great, and the confusion which has resulted, and is further resulting, from its loose employment is so serious, that we shall henceforth restrict it to the original purpose for which Bouguer introduced it, viz., for defining the point in an axis of equilibrium which limits the height to which the centre of gravity can be raised while the equilibrium remains stable, and which is the point of intersection of an axis of equilibrium by a vertical through a centre of buoyancy situated very near to the said axis. But points of intersection of verticals through adjacent centres of buoyancy—whether situated near to the metacentre, and lying therefore in Bouguer's *metacentrique*, or situated anywhere else — we shall define as *movicentres*, and their locus as the *movicentric*. It is almost as desirable to employ a new designation for Bouguer's *metacentrique* as for his extension of *metacentre*, because a clear and sharp distinction is much needed between the locus which we designed the "movicentric," and a curve now much used (as will be hereafter seen), viz., a locus of metacentres due to varying draughts of water. Henceforth, in the works of all those writers on naval architecture who may see fit to adopt this nomenclature, there will be one definite meaning for the word "metacentre;" one as definite for the words "curve of metacentres" or "locus of metacentres;" and the "movicentre" and "movicentric" will be no less clearly understood. There will be no objection to regarding "metacentre" as a "movicentre," or rather as a point in the "movicentric;" it may sometimes be convenient so to speak of it; but all *movicentres*, or all points in the *movicentric*, which are situated in axis of equilibrium, will be *metacentres*, and only those so situated will usually be so called. The use which I have made of the word "metacentre," in reference to Fig. A, is perfectly correct, M being there a true metacentre because situated in an axis of equilibrium, and limiting the height of the centre of

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gravity if the equilibrium is to be stable. It indicates unstable equilibrium in this case, however, as the centre of gravity of the body is situated above it.

M. NORMAND regretted to observe that most excellent things connected with shipbuilding, such as the system of metacentres, which had rendered so good service to shipbuilders in the past, were so unjustly depreciated and pulled down. He rejoiced to see the attention that was being directed to metacentre now. Formerly, ships were almost uniformly built of a certain kind, and which style made the metacentre to stand as a true and almost invariable centre, about which the whole machine was hung and revolved, not only in all the states of the ship in still water, but also when borne by most agitated waves, and at the different immersions to which the ship might be loaded. They would recollect that most of the ancient ships had their sections in the range of extreme loads and inclinations as if drawn from a central point, and this point was the mean centre of suspension, or metacentre. Therefore, the metacentre should be considered as one point round which the whole fabric moves, not only in smooth water, but also in the most disturbed water. This principle was of the greatest interest to shipbuilders to preserve and fully appreciate so as to be aware of the value of the different effects of the waves, and whether, as it was wrongly affirmed by some theorists, the ship independent of the action of the wind had a tendency to incline beyond the perpendicular to the surface of the wave, whereas, on the contrary, a regular and well-formed ship enjoys this admirable property to keep in a mean position between the vertical and the perpendicular to the surface of the wave. He had been connected with the study of Winan's Spindle or Cigar Vessel, in which all the transverse sections were circles described from one longitudinal axis, which was therefore the absolute and unique metacentre of the ship, at all loads and inclinations. Although possessed of small metacentric height it was a secure vessel and most singularly indifferent to the actions of the agitated sea. It was now most important to know whether, in regular and normal ships, the change of height in the

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metacentre was of so great importance as it had been asserted. They had been all considering the diagrams published, that had been leading to different cases of metacentric height, and it was found that in those ships the variations in the height of metacentre, even considering the drop at the launch displacement, which was not of any importance in the life of a ship, was quite inconsiderable. The variations in the "Daphne" metacentre did not go beyond one foot; that is, one foot out of a height of twelve or thirteen feet from the base line; and in the larger ships, considered in the same case, it did not amount to any more. In conclusion, he remarked upon the extreme degrees of stiffness which appeared in some ships, owing to their square and angular shape. He knew that those ships had got recently a surprising and most unexpected patronage, and had been advocated in a very remarkable manner; but he could prove that it was another and more deplorable mistake to all purposes; he cautioned shipbuilders to remember that the shapes which might do very well for the building of houses did not necessarily commend itself to the shipbuilder. He knew that their love for progress would lead them to the due consideration of new systems; but still they ought to preserve the old, proved methods of ascertaining the qualities of a ship; and he asked them to continue, along with the new methods, those which had long been trusted as safe, and had been tested by long experience. In conclusion, Sir E. J. Reed's proposal not to consider any more the totalisation of the successive changes brought in the positions of the centre line of displacement by the accumulated inclinations, but only all these separate and confused actions in themselves, is suggested by no indication of any advantage or even of any possible employment of the new system; the present satisfactory and universally admitted terminology could not be disturbed without at the least introducing in naval science, a deplorable confusion.

Mr GEORGE THOMSON said, with reference to M. Normand's remarks about the circular form of ship, that it was well known that the old type of ship, with great tumble home, it was simply done to clear the upper works from the smoke and fire of the guns

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below, and for better range when the guns were shifting. That would not do for modern vessels, for the engines had also to be carried in excess, and the present type gained stability beyond those, as they heeled over. A flare out did no harm, in his opinion, to a merchant ship.

Mr S. N. BARNABY, by invitation, supplied the following description of his mechanical method of constructing curves of stability :— It is assumed that the position of the centre of gravity of the ship has been obtained by the usual process of inclination. The method of procedure is as follows :—Prick off on a sheet of drawing paper from the body plan of the ship on a scale of, say, a quarter of an inch to a foot, any convenient number of immersed sections taken at equal intervals apart. The water-line up to which the immersed sections are taken should, of course, be that at which the centre of gravity was found. Cut these sections out and gum them together in their relative positions. Care must be taken to spread the gum thinly and evenly. It is not necessary that the sections should be gummed all over. If a stencil plate be prepared with several holes symmetrically disposed in it, a gum brush passed over it will do all that is required. The weight of the block of sections thus obtained represents the displacement of the ship. By suspending it from two points, and taking the intersection of two vertical lines through the points of suspension, the position of the centre of buoyancy is obtained. Draw on the body plan a number of water lines at various angles of heel, all the lines intersecting at the centre of the upright water-plane. Cut out as before the immersed sections corresponding to the first inclined water-line and gum them together. The actual floating line at this angle of heel may be obtained by cutting off parallel slices from the tops of the sections until the weight is the same as those in the upright position. In ordinary merchant steamers, with moderate freeboard, the inclined sections would be less in weight than the upright sections at any considerable angle of heel. It is, therefore, necessary in cutting out the section to leave a margin above the inclined water-line. Find the centre of buoyancy as before by suspending them from two points.

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The distance between a vertical line drawn through the new position of the centre of buoyancy thus found and a vertical line through the centre of gravity of the ship is the leverage at which the weight of the ship acts to right herself at this angle of inclination. If this process is repeated for each angle a complete curve may be obtained. The intervals between the angles might be 15 degrees for practical purposes. I am able to recommend this plan very confidently as before its publication I had an opportunity of putting it thoroughly to the test. My letter was put into the hands of an ordinary, careful, and painstaking draughtsman who had never before been engaged upon calculations of stability. The drawings of a ship of 1100 tons displacement upon a scale of a quarter of an inch to a foot were given to him, and he was told where the centre of gravity was situated as ascertained by an inclining experiment. In 22 hours he produced a complete curve, having in that time found the righting force of the ship at every 10 degrees of inclination. A curve was then independently calculated, when that produced by mechanical means was found to exactly coincide with it.

The discussion was then again adjourned to next meeting, to enable Mr Biles, who was unable to be present, to reply.

The discussion of this paper was resumed on the 22nd January, 1884.

Mr J. H. Biles, in reply to the remarks made in the discussion of his paper, said that Mr Laurence Hill advised them to err on the safe side. That was not an economical principle, and was not always a safe one. It was better to know the danger point, and to keep clear of it by a uniform, reasonable, and known amount, than it was to assume a danger point and then take a wide margin, which must not only cover ordinary contingencies, but must cover our ignorance of the subject. Mr Hill said that the recent talk about metacentres had led the public to expect that a ship should be successfully launched under any circumstances. That was certainly a reasonable expectation on the part of the public, and if a greater degree of certainty could be insured by a study of metacentres, it was

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probable that the public might not only expect that study but might insist upon it. Mr Hill regretted that stevedores and captains had so little knowledge of stability, and could so readily upset the nicely arranged plans of the builders. That undoubtedly was a matter of regret, but Messrs M'Millan, of Dumbarton, in their dual capacity of builders and owners were setting a good example in placing a very simple means in the hands of their captains, of determining whether a ship was safe or not. Mr William M'Millan had made the remark, "that after long experience he had come to the conclusion that shipbuilders must be guided by practical common sense as well as theory." Mr Biles thought it was impossible to make use of theory unless it was guided by practical common sense, and that those people who took most value out of theory were those who had the most practical common sense. He thought that if for curve of stability they substituted the words "full knowledge of the stability" of a vessel, there would not be so many ready to say that a curve of stability was unnecessary. That was primarily the object he had in view in his paper, and by endeavouring to find out how much stability a vessel had at other positions than the upright, and the relation these quantities had to the metacentric height. From a fair amount of data of this kind it was easy to predict the amount of stability from the metacentric height alone, and conversely to judge how much metacentric height was necessary at launching, and therefore he agreed with Mr M'Millan; but he went further, and said that it was impossible to know what was a sufficient metacentric height unless they knew the stability at other positions than the upright. He had to thank Mr Purvis for the information he had given them on the subject, and he hoped that the work on which he was engaged for the Messrs Denny, in order to determine a curve of stability without the labour of calculating it accurately, would be successful, and that if the Messrs Denny would publish the results, it would be of very great interest to all shipbuilders. He thought that M. Normand had missed the point of Sir Edward Reed's report, so far as the following applied to it:—"It had been once more demonstrated that the only circumstances in which a ship

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might be met with a reducing or vanishing stability at great inclination was in the case of a small proportion of freeboard, which evidently could not happen with a ship in launching condition." Now, he thought if anything was clearly shown in Sir Edward Reed's report, it was that with a high freeboard and light draught they would have a very small amount of stability. He thought that if M. Normand would look into the subject further, geometrically, he would find that with a light draught and a high freeboard a vessel was in a worse condition than a vessel with deep draught and small freeboard. That had been pointed out by Professor Elgar and Mr John, who showed that the moment of stability of a ship, whether she was floating upright or upside down, was the same, and that the quantity that tended to destroy that stability must be greater in the upright position, with a light draught, than upside down; and, therefore, there must be less stability in that condition. There was one thing that M. Normand was precise upon, an amount of metacentric height that he would consider sufficient, namely, two feet. He (Mr Biles) also thought two feet was sufficient, but if M. Normand had to pay out of his own pocket money to give a ship two feet of metacentric height instead of only one foot at launching, by putting ballast on board, he thought he would hesitate a little. Mr G. L. Watson had made a remark about preferring to pay to have ballast put in rather than to pay to have the stability calculated, which he himself had a doubt about; but, perhaps it was better not to say any more about it than this—that it was better to know the danger point, and to keep clear of it. Mr George A. Agnew had produced a very interesting curve of stability which referred to a vessel built by Messrs R. Napier & Son. It was a fine ship, with two feet two inches of actual metacentric height, and yet apparently had not stability enough. This curve seemed to some extent to invalidate what he had said with regard to one foot of metacentric height being sufficient; but he did not think that a ship was necessarily insufficient in metacentre that rolled 15 degrees. He could not understand what force had made that vessel so to roll. He had no doubt that Mr Agnew was right in his statements, but they

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were inexplicable, so that he was inclined to adhere to one foot of metacentric height. With respect to Mr Robert Napier's and Professor Thomson's remarks, which principally bore upon a sentence in his paper in which he had not very precisely made reference to the "stiffness" of a ship as affecting her behaviour at sea. As his language had not been very precise, and as the remark was made in reference to something outside the subject of the paper, it was perhaps not worth while discussing the point. He would like to say, however, that he did not in the slightest disagree with what Mr Napier and Professor Thomson said in regard to the "stiffness" of a ship, in relation to her behaviour at sea.

The PRESIDENT said they had heard the paper read by Mr Biles, which had been on a very interesting subject, and at a very interesting period. Had Mr Biles brought his paper before them some few years ago it might have made their shipbuilders somewhat more wary, and more sure that they had a proper centre of safety before they attempted to launch their vessels. He thought that Mr Biles had answered all the questions put to him very ably, and that he had made the most of the points raised, and therefore was justly entitled to a hearty vote of thanks, as well for his paper as for his remarks in reference to the discussion upon his paper.

The vote of thanks was heartily awarded.

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20th November, 1883. Mr JAMES REID, President, in the Chair.

The Chair of Naval Architecture.

The PRESIDENT said that most of the gentlemen present would be aware that the Council had agreed, and were morally bound, to support the institution of a lectureship in anticipation of a Chair of Naval Architecture in the University. Some time ago the proposal was brought before the Institution by the Council and homologated. Mrs. Elder had now given a sum of money to endow a Chair of Naval Architecture in the University, but the Institution was still morally bound, and the Council had agreed, to continue the naval lectureship in connection with the Chair. The Council had resolved to raise a sum of money for this purpose. As the members were aware, Mr. Lawrie had been giving these lectures, and it now fell to the Institution to carry out their original intention of raising the money to endow the lectureship. In the meantime he desired to make this announcement, so that it should be known that the original intention was still to be carried out. Since Mrs. Elder had acted so handsomely in connection with the endowment of the Chair at the University, he thought the Institution could not do less than endow a lectureship. It was believed that a sum of about £5000, yielding about £200 a year, would secure a very able man to give the lectures. He thought that the Institution should acknowledge Mrs. Elder's generosity in this matter, and if that was agreed to, the Secretary would prepare a letter to be forwarded to that lady.

The suggestion was cordially agreed to.

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On Whiteford's Double Lock Railway Chair Key and Fish Joint.

By Mr LAURENCE HILL, C.E.

Received and Read 18th December, 1883.

THIS key and joint is the invention of Dr James Whiteford, of Greenock. It consists in substituting an iron combination key instead of the wooden keys at present in use. The iron model shows the keys fitted to a chair used by the Caledonian Company, and now on trial by them, and the wooden one shows a fish joint now on trial by the South-Western Railway Company, on which line the keys have now been in use at Prince's Pier Station for more than a year. At the same place the fish plate has been down for the same time, and these latter, although with turned rails (which, therefore, do not properly fit the chairs) have, nevertheless, stood the test well,—and it is severe, they being placed at the foot of a steep gradient. One of these joints is placed 47 inches from the sleeper on the one side, and 39 inches on the other side from the nearest sleeper, or nearly double the distance allowed with the common joint, thus greatly increasing the severity of the test.

The *modus operandi* of keying the rail to the chairs, and of fixing or closing the joint, are the same, and are shown by the drawing. (See Fig., p. 66.)

The key, you will observe, is in two parts. The one half C next the chair is cast to suit the chair, and has two small cheeks similar to an ordinary axle bush, their use being to hold the key in its place. The other half D, or key proper, has the one side cast to suit the section of the rail, and the other side to fit against the other half which goes next the chair. The keys have little or no taper, and are cast so as to be put in place by hand, easily, without any slack, and the tightening or wedging is effected by using two or more thicknesses

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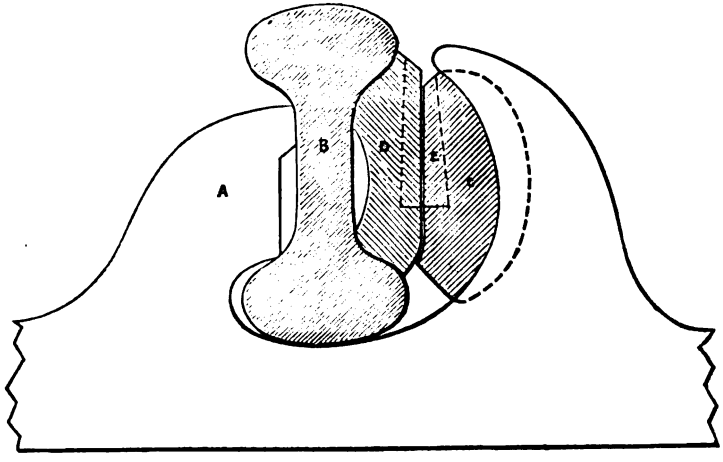
of common brown paper, placed between the chair and the bush-shaped or outside half of key. When the *key* half is entered, a few blows of a *hand* hammer are sufficient to drive it home. On the sides where the two halves meet a small tapered recess is cut. The key is driven up until these recesses are opposite, and a small split pin is dropt into the recess as security against the *possibility* of the keys slipping out of place, either from the expansion of the rail or the jarring of the train. To release the keys, the split pin must first be drawn up.



But, in practice, it will be found that, so far as the keys are concerned, the split pins are hardly necessary, as the brown paper furnishes such an easy mode of making a perfect fit, *all over the bearing* surface, that there is no tendency of the key to move, and their parallelism also, of course, materially aids this. At all events, for 54 weeks, these keys have been in use, both summer and winter, and have shown no symptoms of moving. Wooden keys, on the contrary, would require, in similar situations, to be continually tightened up, and require renewing with new keys every 2½ years.

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The iron key does not appear to require tightening, and, from the nature of the material, ought to last a life-time, or, say, 40 or 50 years, and should be *then* worth half-price, or, at any rate, the price of cast-iron of the then period.

The fish joint, you will observe, is just an enlarged or lengthened out chair, with the keys sufficiently long to allow the chair and wedge to grip enough of the railway bar ends. The only other addition which the fish joints have over the keys and chairs is that there are two short pins which lie in holes punched in the railway ends and also in recesses cast in the chairs, the use of which are to prevent the expansion of the metal in summer from opening the joint. The holes in rail ends are punched, as usual, slightly *oblong*, to admit of the usual expansion and contraction.

The first cost is against the improved key, as the common wooden ones cost little over ¾d a piece, whereas the iron ones will cost from 3d to 5d, according to price of metal; but, while the former requires to be renewed every two or three years, the latter last twenty times as long. Setting aside interest in either case, and taking a period of twenty years instead of forty, the cost of providing wooden keys appears to be ¾d × 7 (or the renewals in twenty-one years) = ⅓d, or over three times the cost of *iron* keys for the same time. There are about 8,000 keys in a mile of double railway. This shows a large saving in material in twenty years in favour of the improved key; but this is only a *part* of the saving, as the iron keys will save largely in *supervision*. The wooden keys require somewhat over six hours per day of a man's time for inspecting and tightening up each two miles of double line of railway, and of this we calculate that three-fourths would be saved by the use of iron keys. Besides this, there is a saving in extra labour during the "dry" seasons, to keep the keys in position, when occasionally almost the whole time of two or three men is required for this purpose for every couple of miles.

Another great advantage is the greater security of permanent way—preserving the gauge—and greater security of rolling stock as a consequence.

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In the after discussion,

Mr BARR asked whether there was any hand-fitting on the key, or was it merely a casting? and whether it was not the case that the paper would soon disappear from the joint?

Mr HILL replied that it was merely a casting. The key had only been 54 weeks in use. It was fixed with only three or four plies of brown paper, which were protected from the weather. With an exact casting the use of one additional ply of paper had split the chair. He had no doubt, however, that the railway workmen would soon get into the use of these chairs.

Mr R. DUNDAS said that if it was not the rule of the Institution to postpone remarks on a paper read at one meeting till the following meeting, he would make a few observations on the paper then.

The PRESIDENT said that he might state, for the information of members, that a paper might be read and discussed the same night, but the discussion must be continued till the following meeting before it was finally closed.

Mr DUNDAS said he had put down about a score of the keys described on the Caledonian Railway, on the south loop line at Rutherglen, used for goods and mineral traffic, and in advance of about 50 malleable iron buckled sleepers; but they had been too short a time laid for him to say much about them. With regard to the joint, he did not think there was so much advantage in it. It was but the old joint chair with an iron key in it, instead of a wooden one. No one who had travelled on a fish-jointed railway, and who happened to go on to a railway with the old joints, but would soon find out the difference. The ear alone would quickly indicate this. There might, however, be something good in the key, although an objection that appertained to it was that two hard substances came together — viz., cast iron and steel, which were bound to wear and to get loose. He thought the clattering noise they would soon make would be disagreeable to travellers; but he did not wish to express a dogmatic opinion in the meantime. He would give them a fair trial, and after they had been in for three or four months, and had so far given satisfaction,

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he would transfer them to the passenger line and leave them there for a few months, and so be able to give an opinion upon them that might be of some use. He thought Mr Hill was rather mistaken with regard to the duties of the platelayers. Their duties did not merely consist in looking to the joints and seeing that the plates and keys were all tight, and that the nuts of the fish bolts were all secure; but they had also to see that the road was otherwise in good order. Even supposing that there was nothing that could go wrong with the fish plates, bolts, and keys still no railway company could dispense with the necessity for having men on the line regularly to pack the sleepers and see that everything was correct. A rail or chair might break and part of the road might be disturbed, so that it was absolutely necessary, apart from the joints or keys, that the platelayers should go along the line regularly. Of course, if the fastenings were better they might spend less time in their duties; but still they must not give up their inspection if the line was to be kept in good order.

Mr HILL said he did not propose to do away with railway inspection.

The PRESIDENT said that no doubt different lines required different supervision, as some lines had more traffic than others.

Mr W. R. M. THOMSON remarked that by the improved railway chair key there was greater leverage hold obtained upon the rail than usual.

Mr S. G. G. COPESTAKE said, as one of the travelling public, he would like to remark that he was afraid this key was tending to lead them back to the old solid form of railway, when there was no medium between the ballast and the rails. He thought wood had a softening effect between the road and the passengers, and he would be sorry to see it removed. No doubt the two metals would wear loose, and therefore he thought it better to keep to the wood, more especially as the latter was more comfortable for the traveller.

Mr HILL said the chair was almost entirely protected; it was almost entirely on the sleeper.

The discussion was then continued till next meeting.

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The discussion of this paper was resumed on the 22nd January, 1884. In reply to the invitation of the PRESIDENT,

Mr LAURENCE HILL said he had very little to add to the paper. He wished to state that he had found that the cost of the iron keys would be less than was given in the paper, the cost of the metal and the weight of the keys being less than he had expected. With castings at £4 10s per ton, and allowing 20 keys to the cwt., the cost of keys would be 2½d each, and allowing them to last only 20 years, their use would effect a saving of £130 in that time, for each mile of *double line*, exclusive of the value of old material. According to the statement of the plate-layer of the South-Western Railway a man's time for six hours daily was required to examine and tighten the wooden keys, on any two miles of double line. Now though most of this might be saved, if only the half were saved, this shows a further saving of £7 to £8 per annum, or, say £140 in 20 years; and this, with £130 saved in material, shows £270 as the beneficial result in 20 years for every mile of double railway where the improved iron keys were used. Besides the trials Mr Dundas was making at Rutherglen, he was also about to place a few of those chairs on the Greenock section; and no doubt Mr Dundas would give them every justice, and by-and-by add his testimony to their value.

A hearty vote of thanks was then passed to Mr Hill for his paper.

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On the Compound Engine, viewed in its Economical Aspect.

By Mr R. L. WEIGHTON.

(SEE PLATES IV. AND V.)

Received 30th August; Read 18th December, 1883.

THE general problem of the economical utilisation of the expansive force of steam has in the meantime been reduced to that narrower question—the compound engine and its design. Experience has shown that all engines constructed on this principle do not attain to the same degree of efficiency. A great many show no higher economy of fuel than the old single cylinder engine. A *considerable* number are far more extravagant than a fair specimen of the old type. In a recently issued report by the chief engineer to the Boiler Insurance Company, Manchester, will be found a detailed account of some carefully conducted trials on the comparative efficiencies of simple and compound engines. From that report it appears that one engine, rejoicing in the title "compound," was making away with coal to the extent of 6·83 lbs. for every horse-power indicated (see Fig. 1), while a "simple" neighbour was content with the very moderate allowance of 2·35 lbs. for the same energy exerted! The mere fact of an engine being "compound" is therefore no criterion of its economy. Apart altogether from differences in workmanship, which may explain a deal of the above waste, it may be worth while to inquire into the effect on efficiency of differences in design and proportions; to endeavour to ascertain the conditions under which, in varying circumstances of boiler pressure, and of total expansion, the compound engine attains its maximum efficiency.

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Steam is of the nature of a gas. Gases may be said to be of two kinds—perfect gases, and those which are not perfect. A perfect gas may for our purpose be defined as a gas which can be compressed or expanded without liquefying during the process, and the relation between pressure and volume is expressed by Boyle's law, viz., PV = a constant quantity, the temperature remaining constant. Superheated steam, expanding in a well-jacketed cylinder, has been found by experiment to sensibly follow Boyle's law, so long as the temperature is kept nearly constant by means of the jacket. Ordinary saturated steam is an imperfect gas, and when expanding in a cylinder it does not follow Boyle's law, because its temperature is falling; and besides, it is found that, in order to do work while expanding between two temperatures, saturated steam must part with more heat than is represented by the difference between these two temperatures. But it must continue to exist as "saturated," and in the absence of an extraneous source of heat—such as a jacket—it can do this only by abstracting a portion of its own latent heat, and so a definite quantity liquefies. For saturated steam expanding in an unjacketed cylinder, Professor Rankine has from experiment deduced the following expression, which will be found to give a good average representation of the actual process, viz., $PV^{\frac{10}{9}}$ = a constant quantity. The same authority gives an equation for saturated steam when the cylinder is jacketed, viz., $PV^{\frac{11}{10}}$ = constant. Here the index approaches unity, and the curve drawn in accordance lies between the $\frac{10}{9}$ curve and the pure hyperbola. These curves are represented in juxtaposition in Fig. 2.

From Regnault's tables it will be seen that the total quantity of heat which must be spent in order to produce 1 lb. of steam varies very little within the ordinary limits of pressures. But be it likewise noted that while the total amount of heat per lb. of steam increases very slowly as the pressure rises, the weight of a cubic foot of steam increases very fast as the pressure rises. Hence the economy of high pressures of themselves is not so obvious as would appear at a casual glance. Steam of 50 lbs. absolute pressure contains 1199 units of heat per lb., and weighs ·1202 lbs. per cubic

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foot. Steam of 150 lbs. pressure contains 1222 units, and weighs ·3377 lbs. per cubic foot. One cubic foot of steam at 50 lbs. pressure contains therefore 144 units of heat, while one cubic foot at 150 lbs. pressure contains 415 units. But 1 cubic foot at 50 lbs. represents 50 foot lbs. of work without any expansion, and 1 cubic foot at 150 lbs. represents 150 foot lbs. without expansion. Hence 50-lb. steam expends $\frac{144}{50} = 2{\cdot}88$ units of heat for every foot lb. of work done, and 150-lb. steam expends $\frac{415}{150} = 2{\cdot}77$ units for every foot lb. of work done. There is no great economy shown here by the adoption of a threefold pressure. The secret of the economy must be sought elsewhere, and is to be found in expansive working With 50 lbs. pressure, an expansion of 4 times would be about the limit. With 150 lbs. we might expand 10 or 12 times, and still have a margin.

The diagram, Fig. 2, Plate IV., exhibits the advantage of working expansively, and a glance serves to show the immense saving which is represented by a difference between 4 and 12 as ratios of expansion.

Let it be assumed for simplicity that steam is used of 100 lbs. absolute pressure, corresponding very nearly to 85 lbs. boiler pressure. The end aimed at is of course to extract the utmost possible amount of work from the steam, consistent with a reasonable weight of machinery. With this object the steam will be expanded until its terminal pressure is say 8 lbs. or 10 lbs. above a perfect vacuum. The temperature of saturated steam of 100 lbs. = 328 degrees Fahr. The temperature of the same steam at 8 lbs. = 183 degrees. The difference, 145 degrees, represents the range of temperature which a cylinder undergoes per stroke were we to attempt expansion in one cylinder only. To get some definite notion of what this entails, let us assume a cylinder of 1 square foot area, into which 1 cubic foot of steam is admitted at 100 lbs. pressure. The surface of the cylinder which is exposed to the steam when just fully admitted—*i. e.*, at the cut-off point—will be 13·5 inches (diameter to give 1 square foot) × 3 × 12 inches + 2 × 144 inches = 774 square inches. Assuming the alternations of temperature to penetrate $\frac{1}{16}$ inch into the

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metal, we get 774 × ·062 = 47 cubic inches = 12 lbs. of metal to be raised through 145 degrees Fahr. each stroke. The units of heat required to do this will be 12 × 145 × ·114 (specific heat of iron) = 198 units. The total heat in 1 cubic foot of steam at 100 lbs. = 1213 × ·2307 = 279 units. The amount of steam required to heat the cylinder at each stroke is therefore $\frac{198}{279}$=·71 of a cubic foot,—*i.e.*, 1 cubic foot goes to do the work, and very nearly $\frac{3}{4}$ of another cubic foot is absorbed by the cylinder. There can be no doubt that *some* quantity of this heat will re-appear in the work done during expansion, by its effect in maintaining the temperature somewhat. But the saving in this direction cannot possibly amount to three-quarters of the whole work of admission. As matter of fact, the expansion curves of diagrams, taken under similar conditions to the above, do *not* indicate that any appreciable quantity of the heat absorbed by the cylinder walls is restored during the progress of expansion. Hence the origin of two cylinders to diminish this temperature range. The primary idea, therefore, in the compound engine is the necessity for *expanding* the steam to its utmost, combined with the equal necessity for avoiding excessive alternations of temperature in the boundaries of the cylinder. How, then, are we to employ our steam to the best advantage? By expanding it as far as possible in two or in more than two cylinders, according to the pressure we use, and the degree of economy we desire. At present I confine myself to the two cylinder engine and steam at 100 lbs. absolute pressure; but before taking leave of the subject, I shall have a few observations to make relative to the three cylinder type of engine and steam of much higher pressure.

The following question here occurs, on the answer to which the whole of our conclusions must depend:—How are we to find the power which, in a proposed engine, will be developed in each cylinder? Or, in other words—for it ultimately resolves itself into this—By what method can the mean pressure in each cylinder be calculated? In the case of a single cylinder engine, the computation of the expected mean pressure is a very simple and obvious process; but it becomes, in the case of two cylinders, which are at one time

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in communication with each other, and which are at another time shut off from such communication, a rather intricate operation. For the sake of simplicity in illustration, let us assume that the steam we are dealing with at present is superheated, and working in a jacketed cylinder. It will consequently follow Boyle's law, very nearly,—*i.e.*, the expansion curve will be a simple hyperbola, PV = constant. The symbols to be used may as well now be explained.

P_1 = absolute pressure of the steam.

P_2 = average receiver pressure.

P_3 = average back pressure opposing the low pressure piston.

r = ratio of expansion in high pressure cylinder, and = $\frac{\text{stroke}}{\text{cut off}}$.

R_1 = ratio of expansion in low pressure cylinder, and = $\frac{\text{stroke}}{\text{cut off}}$.

R = ratio of cylinder capacities = $\frac{\text{low pressure capacity}}{\text{high pressure capacity}}$, or when the stroke of both is the same, simply = $\frac{\text{low pressure area}}{\text{high pressure area}}$.

There is an assumption involved in the method I am to adopt which calls for mention. I assume that the receiver pressure is constant during the period of low pressure admission. This, of course, is by no means the actual state of matters. In order to be strictly true, the receiver would require to be of infinite capacity, or, at least, so large that the abstraction from it of a cylinderful of steam would not sensibly affect the pressure of the remainder. But though this be an assumption, it does not affect the conclusions arrived at by means of it, and the only difficulty is to estimate this mean pressure correctly. Under this assumption we have—

$$\text{Mean press. in high pressure cylinder} = P_1 \times \left(\frac{1 + \text{hyp. log. } r}{r}\right) - P_3,$$

and

$$\text{Mean press. in low pressure cylinder} = P_2 \times \left(\frac{1 + \text{hyp. log. } R_1}{R_1}\right) - P_3,$$

But

$$P_2 = \frac{P_1 \times R_1}{rR}.$$

Therefore, we get the mean pressure in both cylinders, in terms of P_1 of r of R_1, of R and of P_3, all known quantities, or easily estimated

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as in the case of P_3, which is generally equal to 4 lbs. Here, it will be observed, I calculate for the mean receiver pressure, as if it were equal to the pressure in the low pressure cylinder at the instant when the low pressure valve is cutting off. For any engine—the indicator cards of which you have before you, and the cut-offs of which you know accurately—if you reckon P_2 by the above process you will get a value which will require to be diminished by about 12 per cent. before you arrive at the same relative mean pressures in the cylinders as the cards show—these mean pressures being calculated by the formula

$$P_1 \times \left(\frac{1 + \log. r}{r}\right) - P_2, \text{ \&c.,}$$

as given. And this 12 per cent. will be found to represent very nearly the drop of pressure in the receiver which is due to clearance and to wire-drawing in the cylinders, and which clearance and wire-drawing is not allowed for in the usual fashion of stating the cut-off by the slide valves. Whether we allow at first for the effect of clearance and wire-drawing by a diminution of r and R_1, or whether we deduct from P_2, found in the usual way, a certain percentage as the result of experience, it is obvious the ultimate conclusion will be in no way affected. The latter course is much the simpler, and is adopted here. In cases where the clearance diverges much from $\frac{1}{13}$ or $\frac{1}{16}$ of the cylinder capacity, or when corliss or equilibrium valves are used, instead of the ordinary slide valve, it will be more accurate to deal with the actual clearance from the beginning of the process, and it will be necessary to make somewhat less allowance for wire-drawing.

For saturated steam expanding in unjacketed or in imperfectly heated cylinders, our formulæ become—

$$\text{Mean pressure in high pressure cylinder} = P_1\left(\frac{10}{r} - \frac{9}{r^{\frac{10}{9}}}\right) - P_2,$$

$$\text{Mean pressure in low pressure cylinder} = P_2\left(\frac{10}{R_1} - \frac{9}{R_1^{\frac{10}{9}}}\right) - P_3,$$

And

$$P_2 = \frac{P_1}{\left(\frac{Rr}{R_1}\right)^{\frac{10}{9}}} = \text{mean receiver pressure nearly.}$$

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These equations apply to steam which while expanding is also liquefying, and thereby preserving the relations of pressure, volume, temperature, and density, as given in Regnault's tables, the curve which is described being, as already stated, $PV^{\frac{1}{8}\rho} = \text{constant}$ or $P \propto V^{-\frac{1}{8}\rho}$.

As the use of these formulæ is an extremely tedious process, I was under the necessity of calculating them out for values of r or R_1, ranging from 1·2 to 5·2, and differing by $\frac{1}{10}$. These are given in Table VIII., and along with them also values of $r^{\frac{1}{8}\rho}$. Their application will appear further on.

We are now in a position to consider the conditions which make for perfection in the compound engine. A clear and definite apprehension of *what should be* is the indispensable antecedent of every rational attempt at improvement. The end to be aimed at, though, of course, it can never be absolutely realised, is to convert the whole available heat in the steam into work. In the case before us, this will be effected if we expand our steam from its initial pressure P_1 down to zero, and that without loss during the process. With steam as a medium, this would be perfection. We would utilise all the sensible heat, and lose only the latent heat. Our *engine* would still be far, very far, from perfection in an absolute sense, which can only be attained by the utilisation not only of the sensible, but likewise of the latent heat. Until, however, some benefactor of his kind stumbles upon a more perfect medium than steam, we must content ourselves with the realities of the present, and refrain from dreams of the possibilities of the future.

But we cannot expand down to zero: P varies inversely as V. If P disappears, V becomes infinite—*i.e.*, our cylinder would require to be of infinite capacity. And, besides, owing to imperfection of mechanism in air pumps and condensers, a perfect vacuum cannot be attained. The loss by back pressure may, therefore, to a certain extent, be looked upon as inevitable. I assume this loss to be represented by a pressure of 4 lbs. per square inch of piston—$P_3 = 4$. Sometimes it may be less, and often it is greater. Another loss which is inevitable in the engines of the present day is that due to

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the clearance in ports and cylinders. The expansion of a given volume of steam (including the clearance volume) from one pressure to another pressure, without any loss in the process of expansion, may be taken as a definition of perfection which will be sufficient for our present purpose. *Not* but what the elimination of both clearance and back pressure would materially increase economy, but, admitting them as necessary evils, how will we make the most of the capabilities of our present type of engine? Let the initial pressure, $P_1 = 100$ lbs. per square inch absolute, and let this be expanded down to 4·5 lbs., which is almost to the back pressure. We have here $100 - 4{\cdot}5 = 95{\cdot}5$ lbs. per square inch which has disappeared. Now, in order that *all* this may appear in the form of work done, it is necessary that, *while expanding*, the steam should spend its pressure against a *moving piston*. If pressure falls in any way except by reason of increase of volume due to piston motion, there is a disappearance of available energy as represented by pressure. But no *work* is done except by piston motion, therefore, there is a disappearance of available energy which has no equivalent in work done.

While the steam expands from 100 to 4·5 lbs., there should therefore be no decrease of pressure except behind a moving piston; and this is one of the conditions of perfect efficiency. This object would be attained were it possible, without loss due to temperature range, to use *one* cylinder only for our purpose. We should get a diagram complete, as outlined on Fig. 3, Plate IV. The only losses would be those due to clearance, to back pressure, and to non-expansion of the steam to zero. But this diagram could not be obtained from a single cylinder engine for reasons which I have already indicated. We are then constrained to use two cylinders The initial volume and the limits of pressure being the same, the diagram (Fig. 3, Plate IV.) will be the correct *expanded* diagrams for a compound engine, if we except the additional clearance of the low-pressure cylinder. To render this a complete diagram for the two cylinders, all we have to do is to draw a horizontal line at a height above the zero line equal to the mean receiver pressure,

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To do this, we must know either the rate of expansion in the high pressure cylinder (r), or the ratio between the cylinders (R). In order to equalize the powers in the two cylinders, r will be equal to 5, and R will be 4·5. The mean receiver pressure (P_2) will then be $= \frac{180}{9} = 20$ lbs. P_2 drawn at this height will cut the curve in B, and $\frac{CD}{AB}$ will equal $4{\cdot}5 = R$, the cylinder ratio. The high pressure diagram lies above, and the low pressure below AB. This expanded diagram fulfils the conditions for perfect action in a compound engine. These conditions will now be seen to be :—(1) Expansion down to the back pressure before release takes place ; (2) Equal powers developed in each cylinder ; and (3) The rate of expansion in the low pressure equal to the ratio of the cylinders — $R_1 = R$. As regards the *first* of these conditions, expansion down to the back pressure is obtained simply by either an early cut-off in the high pressure, or a large ratio of cylinders, or by both combined. As regards the *second* condition, equality of the powers in the cylinders is governed by the relation between the expansion in the high pressure (r) and the ratio of cylinders R, or by the mutual relations of r, R_1 and R, where R_1 is not equal to R. With respect to condition *three*, the equality of R_1 and R secures that during expansion there shall be no drop of pressure anywhere, except behind a moving piston—that there shall be no receiver drop.

The remainder of this paper will be devoted mainly to exhibiting : I., The proportions and adjustments necessary to fulfil the foregoing conditions as to quasi-perfect action ; II., The proportions and adjustments which, for the sake of certain advantages of a practical nature, depart to a definite extent from these conditions ; and lastly, The relative economy as regards consumpt of fuel of the various examples brought forward.

I. *Perfect Action.*—On the assumption that the horse power in each cylinder must be equal, and that there must be no loss by drop of pressure in the receiver, the problem becomes—What is the relation between P_1, r, and R—"R" being the ratio of the cylinders and also the expansion in the low pressure ?

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To begin with, let us take superheated steam and the hyperbolic curve.

High pressure mean pressure $= P_1 \left(\frac{1+\log. r}{r}\right)$ — receiver pressure.

Low pressure mean pressure = receiver pressure $\left(\frac{1+\log. R}{R}\right) - P_3$.

But the receiver pressure $= \frac{P_1}{r}$ (there being no receiver drop), and the high pressure mean pressure = R × low pressure mean pressure (to give equal powers); therefore,

$$P_1 \left(\frac{1+\log r}{r}\right) - \frac{P_1}{r} = R \left\{ \frac{P_1}{r} \left(\frac{1+\log R}{R}\right) - P_3 \right\}.$$

i.e.,
$$\frac{P_1 + P_1 \log. r}{r} - \frac{P_1}{r} = \frac{RP_1 + RP_1 \log. R}{rR} - P_3 R.$$

$$\frac{P_1 + P_1 \log. r - P_1}{r} = \frac{P_1 + P_1 \log. R}{r} - P_3 R.$$

$$P_1 \log. r = P_1 + P_1 \log. R - rP_3 R.$$

$$\log. r = 1 + \log. R - \frac{rRP_3}{P_1}.$$

This is the equation which expresses the relation for superheated steam expanding according to Boyle's law. It is not in a very convenient form, there being an unknown quantity on each side. But it cannot be further reduced, and is to be used by means of the process of trial and error, a process which, for the subject in hand, has to be very often resorted to.

Table No. I. is calculated to satisfy the above equation. I have taken pressures of 90, 100, and 110 lbs. to show the effect which the factor P_1 has upon the proportions. There are 4 degrees of total expansion shown for 90 lbs., and 3 degrees for each of the other pressures. In No. I. at each pressure the total degree of expansion would appear to be at a maximum. To increase it would very likely entail a greater loss through cylinder cooling than is gained by the additional expansion. In No. I., pressure 90 lbs., the ranges of temperature are, in high pressure, 95 degrees, in low preesure, 72 degrees.

In No. I., pressure 100 lbs., these are, high pressure, 100 degrees low pressure, 75 degrees.

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In No. I., pressure 110 lbs., they amount to, high pressure, 104 degrees, low pressure, 77 degrees.

In practice, a range of temperature of from 90 to 100 degrees is seldom or never exceeded, at least in compound engines. There is, however, a want of reliable experiments on this particular point, and in the absence of such, it were difficult to say what might be the most advantageous degree of expansion with any given pressure.

In order to show the limits within which we can operate, I carry out the comparison to the point when the cylinders become equal, and the total expansion then is simply what takes place in the high pressure, *i.e.* = *r*. The proportions necessary in this case are shown in line No. 4 for 90 lbs., and in line No. 3 for 100 and 110 lbs. And it is instructive to note that in this case, which is the extreme of small "ratio of cylinders" type of engine, the greater range of temperature occurs in the low pressure cylinder, instead of in the high pressure as in lines No. 1. With 90 lbs. the ranges are, high pressure, 60 degrees, low pressure, 107 degrees; with 100 lbs. these are, high pressure, 60 degrees, low pressure, 115 degrees; with 110 lbs. they are, high pressure, 61 degrees, low pressure, 120 degrees. The best distribution of temperatures occurs in line No. 2, when $r = 4$ in the three cases. With 90 lbs. the ranges are, high pressure, 86 degrees, low pressure, 81 degrees; with 100 lbs., high pressure, 88 degrees, low pressure, 87 degrees; with 110 lbs., 89 and 92 degrees respectively. Here you observe we are still within the ordinary working limits of experience. The maximum piston loads, too, with these proportions are as near equality as it is possible to have them. All things considered, therefore, the proportions under line 2, for pressures of 90, 100, and 110 lbs. respectively, are to be chosen in preference to any others, if the only and sole object is to attain the maximum economy with thoroughly superheated steam employed in well jacketed cylinders.

Figs. 4 and 5, Plate IV., are a diagramatic representation of Nos. 2 and 3, Table I., pressure = 100.

When it is ordinary saturated steam that we propose using in this economical way, the proportions best suited for the purpose can be

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calculated in much the same manner as for superheated steam, only the formulæ are quite irreducible to any simple form of equation. As already stated the mean pressures are found thus :—

High pressure mean pressure = $P_1 \left(\frac{10}{r} - \frac{9}{r^{1.6}}\right)$ — receiver pressure.

Low pressure mean pressure = receiver pressure $\left(\frac{10}{R} - \frac{9}{R^{1.6}}\right) - P_3$.

But the receiver pressure = $\frac{P_1}{r^{1.6}}$ (nearly) (there being no receiver drop), and high pressure mean pressure = low pressure mean pressure × R (to give equal powers); therefore,

$$P_1 \left(\frac{10}{r} - \frac{9}{r^{1.6}}\right) - \frac{P_1}{r^{1.6}} = R \left\{ \frac{P_1}{r^{1.6}} \left(\frac{10}{R} - \frac{9}{R^{1.6}}\right) - P_3 \right\}.$$

This equation expresses the relation between P_1, r, R_1, and R, when $R_1 = R$, which is the case in hand. The proportions exhibited on Table II. are calculated in accordance with this equation. You observe I begin with cylinders of a ratio of 4 : 1, and go down by equal gradations to equality. I do not go higher than 4 : 1 simply because the excessive ranges of temperature in the cylinders preclude the practicability of higher ratios, and I end with equality for the reason before stated—to show the limits of our endeavours. The proportions in line 1 are all feasable. The highest temperature range occurs in the high-pressure cylinder with 110 lbs., and amounts to 96 degrees, which is not outrageously high. All things again considered it would, however, appear that the engines in line 2 would best fulfil our purpose of using saturated steam with the utmost possible economy. In these both the temperatures and the maximum loads approach sufficiently near to equality to obviate any very great loss either by condensation or by heated bearings.

With respect to the ratios of the cylinders there is no fixed relation which is better than any other. Generally speaking the greater the degree of expansion to which it is desired to carry the steam the greater should be the ratio of the cylinders, *i.e.*, the low-pressure should exceed the high-pressure in a greater ratio. No rule can be laid down which would govern all conditions. But in similar conditions as to initial pressure, degree of expansion in each

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cylinder, and in both combined, and receiver capacity, there can be only *one* proportion of cylinders which will give the best result in an economical sense. I have said that the greater the required amount of total expansion the greater should be the difference between the cylinder capacities. Th is, however, is modified and governed by other considerations which operate simultaneously. I refer to the necessity for preserving the equality, or nearly so, of the powers developed in the two cylinders; to the necessity for obviating any appreciable loss in the receiver, and to the necessity—in the case of the *marine* engine—for admitting steam for a considerable length of the stroke in *both* cylinders, to enable the engines to be handled with some degree of certainty and despatch. The engine which, while fulfilling all these conditions attains to the greatest degree of total expansion of the steam, is the engine best suited for the circumstances, and *its* proportion of cylinders will be the best possible proportion.

In the foregoing examples of both superheated and saturated steam we have so proportioned our engines that with varying degrees of total expansion to suit any desired economy, we could secure equal powers in the cylinders only by cutting off steam in the low-pressure cylinder at such a point that its ratio of expansion in the cylinder was equal to the ratio of the cylinders themselves ($R_1 = R$); in other words we employed our steam in the most economical manner known to us, by preventing its falling in pressure while passing from one cylinder to the other. This is what I have called perfect action. But at the higher cylinder ratios, such as 4 : 1, this means a cut-off at $\frac{1}{4}$ of the stroke. Those who are acquainted with the handling of engines—and marine engines in particular—know well the virtues of a late cut-off by the main valves for this end; and a cut-off at $\frac{1}{4}$ of the stroke would be almost prohibitory of reversal or starting within the space of time usually allowed as sufficient for these operations. In the absence therefore of a low-pressure expansion valve, which could be easily thrown out of gear —although I do not see *why* there should not be such a valve for the one cylinder as well as for the other—in the absence of such a

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valve, it behoves marine engineers to cut off steam in the low-pressure cylinder no earlier than is absolutely necessary in the demands of economy.

II.—A glance at any expanded diagram will show that though the pressure is allowed to fall a little in the receiver the resulting loss of work is inconsiderable. This, however, is the case only at the higher limits of cut-off in the low-pressure cylinder. A drop of pressure of say from 10 per cent. to 15 per cent. does *not* entail any very serious loss of work, and the advantages otherwise gained are certainly worth the cost. But in the case of engines working with a fall of pressure of from 40 per cent. to 60 per cent. the loss of work is something which tells very appreciably on the coal consumpt. In our previous calculations we have preserved $R_1 = R$. In allowing a fall of pressure in the receiver we simply make $R > R_1$. But this alteration will affect all the proportions, and the advantage of allowing a slight receiver drop is apparent when we find that the later cut-off in the low-pressure entails a later cut-off in the high-pressure cylinder also, if the respective horses-power are to be kept equal. Allowing therefore for a fall of pressure in the receiver of from 15 per cent. to 22 per cent., I have tabulated on Table III. another series of examples for 90 and 100 lbs. respectively.

The formulæ by which these are calculated is a slight modification on those employed for Table II. They are—

$$\text{High pressure mean pressure} = P_1 \left(\frac{10}{r} - \frac{9}{r^{1.5}}\right) - \text{receiver pressure.}$$

$$\text{Low pressure mean pressure} = \text{receiver pressure} \left(\frac{10}{R_1} - \frac{9}{R_1^{1.5}}\right) - P_3.$$

But,

$$\text{Receiver pressure} = \frac{P_1}{\left(\frac{Rr}{R_1}\right)^{1.5}} \text{ very nearly,}$$

and high pressure mean pressure = R × low pressure mean pressure (to give equal powers); therefore,

$$P_1 \left(\frac{10}{r} - \frac{9}{r^{1.5}}\right) - \frac{P_1}{\left(\frac{Rr}{R_1}\right)^{1.5}} = R \left\{ \frac{P_1}{\left(\frac{Rr}{R_1}\right)^{1.5}} \left(\frac{10}{R_1} - \frac{9}{R_1^{1.5}}\right) - P_3 \right\}$$

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Compare Table III. with Table II., where $R = R_1$. In No. I., 90 lbs. pressure, it will be observed that $R_1 = 3{\cdot}5$, instead of 4, as in Table II. This has entailed a diminution of r from 3·6 to 3, and so on with the others. The proportions are approximating to what is generally met with in every-day practice. But still r and R_1 are higher than we usually find them. The ranges of temperature on Table III. are a *little* more favourable than on Table II., but the improvement in this particular is very small indeed. The further diminution of r and R_1 can be attained in two ways only—either by permitting a still greater fall of pressure in the receiver and a consequent loss of work, or by tolerating inequality to a considerable extent in the power developed from each cylinder. The latter alternative may be looked upon as the lesser of the two evils. It is quite a simple matter to adapt the piston and connecting-rods and crank of each cylinder to the load it is to bear, and, though that load be greater in the one case than in the other, it simply means heavier gear in the one and lighter gear in the other. The total weight will be unaltered.

In Table IV. are given the details for a drop of pressure in the receiver of from 35 per cent. to 40 per cent. There you will observe the values of r and R_1 approximate to what is commonly found in practice. Many engines, however, are working with proportions which allow of a far greater drop than these. It is no uncommon thing to find cylinders of a ratio of 4 : 1 having the steam cut-off at $\frac{3}{4}$ of the stroke of both cylinders. An expanded diagram from engines of this sort is shown in Fig 6, Plate IV. In Table IV. the results for 100 lbs. only are given. This serves the purpose intended, as well as any number of pressures, and, for the sake of comparison, 100 lbs. is retained all through these tables.

I have already said that the mean receiver pressure, as calculated by the foregoing methods, will *not* agree with the actual pressure as given by experiment in diagrams, but that it will exceed the real pressure by about 12 per cent. The explanation I indicated as being found in the operation of clearance and wire-drawing in both cylinders, and these play a very important part in modifying our

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proportions. Supposing then, that the difference is 12 per cent., and this is a mean result which I have deduced from a considerable number of engine trials, and which corresponds, as it happens, to a clearance of from $\frac{1}{15}$ to $\frac{1}{20}$ in each cylinder, and a wire-drawing in high pressure of 15 per cent. to 20 per cent.

Tables V., VI., and VII., are as it were corrections on Tables II., III., and IV. Table V. is the counterpart of Table II., where $R = R_1$, and $P_2 = \frac{P_1}{r^{1\frac{9}{5}}} \times \cdot 88$. Table VI. corresponds to Table III., where the drop of pressure in the receiver, by reason of lateness of low-pressure cut-off, amounts to from 15 per cent. to 22 per cent., and where $P_2 = \frac{P_1}{\left(\frac{Rr}{R_1}\right)^{1\frac{9}{5}}} \times \cdot 88$. Table VII. corresponds to Table IV., where the before mentioned drop is from 35 per cent. to 40 per cent. and where $P_2 = \frac{P_1}{\left(\frac{Rr}{R_1}\right)^{1\frac{9}{5}}} \times \cdot 88$.

It may here be necessary to explain that Tables I., II., III., and IV. are purely theoretical, and are introduced solely for the purpose of showing the necessity for the particular proportions adopted in Tables V., VI., and VII.

Figs. 7, 8. and 9, Plate V., are diagramatic representations of Nos. 1, 4, and 6, Table V., pressure = 100 lbs.

Fig. 10, Plate V., shows the expanded cards which were obtained from engines of a very usual proportion, viz.:—R = 3·28, r = 1·34, R_1 = 1·6. These show a very good result—as good, perhaps, as the proportions admit of.

Fig. 12, Plate V., shows the expanded cards which ought to be obtained from engines designed in accordance with the proportions given in Table V., No. 2, pressure 100 lbs. Compare the small receiver loss in this case with that in Fig. 6, Plate IV.

One word about the receiver of compound engines. It is no unusual thing to hear some unexpected dearth of power attributed now to the largeness of a receiver, and anon to its smallness. Now, if for a moment it be considered on what the mean receiver pressure

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depends, it will be seen that, apart from the fluctuations of pressure which its smallness may cause, a receiver can have no effect on the ultimate power realized. The receiver pressure depends upon the relation of the volume of the low-pressure admission to the volume of the high-pressure release. If those volumes are equal—if $R = R_1$—then the receiver pressure is equal to the high-pressure release. If the low-pressure admission is double the high-pressure release, then the mean receiver pressure is approximately one-half of the high-pressure release, and so on. In short, $P_2 = \frac{P_1 R_1}{rR}$, whatever r or R_1 may be. This for superheated steam. Here the size of the receiver enters not at all into the question. After an engine has got fairly under way, and the steam has had time to adjust itself to its normal working condition, the only effect which a receiver can have is to cause fluctuations in the back pressure line of the high-pressure and in the steam line of the low-pressure, and the amount of these fluctuations will be inversely as the capacity of the receiver. From this it will be apparent that a small receiver will have a tendency to raise, on the whole, the average back pressure line of the high-pressure, and the steam line of the low pressure, thus affecting, to some extent, the distribution of the power—diminishing the high and increasing the low-pressure. The larger a receiver can be conveniently made, the steadier will be the action of the engine. It should never be less than 1½ times the high-pressure cylinder, and it may, with propriety, reach 4 or 5 times that capacity. I refer, of course to engines with cranks at 90 degrees, or thereabout. In the case of engines working on one crank, where both cylinders begin and end their strokes simultaneously, like the old Woolf engine, the smaller the intermediate passages can be made, the better.

III.—Opposite each example there is stated the efficiency and also the coal per indicated horse power per hour. To give these relative efficiencies their full significance, it will be necessary to explain the methods by which they are arrived at. As before, we assume the high-pressure cylinder to be one cubic foot capacity—*i.e.*, 1 foot area and 1 foot stroke. Assume also that the water to

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be used is at an initial temperature of 53 degrees Fahrenheit when put into the boiler. Then we have the efficiency

$$= \frac{\text{Heat utilised}}{\text{Heat spent}}.$$

But the heat utilised is equal to the units of heat converted into work during one stroke, as represented in the mean pressures calculated according to previous methods—

$$= \frac{\text{Mean pressure on high pressure} \times 144}{772}.$$

The heat spent during one stroke will be simply the total units of heat which have been imparted to the steam admitted; and this depends upon the *amount* of it admitted and on its pressure—*i.e.*, it depends upon r and P_1. Now, the amount admitted in lbs. will be the weight of 1 cubic foot at the given pressure P_1 multiplied by the cut-off in fractions of the stroke, which is simply the reciprocal of r; and the heat spent will be this product multiplied by the total heat available in steam at the given pressure. Taking $P_1 = 100$ lbs.

$$\text{The heat spent} = (1213 - 53) \times \cdot 2307 \times \frac{1}{r}$$

$$= \quad 1160 \quad \times \cdot 2307 \times \frac{1}{r}$$

The efficiency therefore (for $P_1 = 100$ lbs.)

$$= \frac{\text{mean pressure} \times 144}{772} \Big/ 1160 \times \cdot 2307 \times \frac{1}{r} = \frac{\text{mean pressure on high pressure}}{1434 \times \frac{1}{r}}.$$

$$= \frac{r \times \text{mean pressure}}{1434}.$$

For a pressure of 90 lbs., the efficiency $= \frac{r \times \text{mean pressure}}{1279}$.

For a pressure of 110 lbs., the efficiency $= \frac{r \times \text{mean pressure}}{1570}$.

In the calculation of these efficiencies, the mean pressure used is not the mean pressure as given by the preceding formulæ. These mean pressures are higher than would be actually registered by engines working to the figures given. They are higher in a certain ratio, which ratio will be approximately constant for them all if the

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conditions of working are similar. From experience, this ratio appears to be represented with sufficient accuracy by ·9. That is to say, the mean pressures, as arrived at by the foregoing methods, are $\frac{1}{10}$ higher than can be looked for in actual working. This $\frac{1}{10}$ is absorbed by clearance, friction in the passages, radiation, and other drawbacks incidental to engines as at present constructed. The amount by which the calculated exceeds the actual mean pressure can be quite readily found in every case in which the requisite data are at hand, by reckoning the mean pressure in the manner shown, and dividing the actual diagram mean pressure by it. The result will be something very near ·9, and this is to be used as a "constant."

To get as near an approximation as possible to the coal consumpt, reduce the theoretical mean pressures to their actual value by multiplying by this constant. The mean pressures and the efficiencies on the tables are reckoned in this way.

It must be borne in mind that some engines will show more than $\frac{1}{10}$ of a deficiency, while others may show less. But, in well-constructed engines, working with no excessive range of temperature in either cylinder, it will be found very near to what is stated.

Coal Consumpt.—1 lb. water evaporated from 53° Fah. to steam of 100 lbs. per square inch absorbs 1213 — 53 = 1160 units of heat. 1160 × 772 = 895,520 foot lbs. The fraction of this which is utilised will be found by multiplying it by the efficiency. Taking No. I. Table II. 100 lbs., the efficiency of which is ·1724, we have 895,520 × ·1724 = 154,397 foot lbs. utilised out of each pound of water used. The pounds of water used per horse-power per hour $= \frac{33{,}000 \times 60}{154{,}397} = 12{\cdot}8$ lbs., and the coal consumed per horse-power per hour $= \frac{12{\cdot}8}{8} = 1{\cdot}6$ lbs. One pound of coal is assumed to evaporate 8 lbs. of water, corresponding to an evaporative efficiency in the boiler of about ·6, which may be taken as a fair average.

For a pressure of 100 lbs. the coal per indicated horse power per hour is therefore $= \frac{1{,}980{,}000}{895{,}520 \times 8 \times \text{efficiency}} = \frac{\cdot 2763}{\text{efficiency}}$.

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For 90 lbs. pressure the coal per I.H.P. per hour $= \frac{\cdot 27685}{\text{efficiency}}$.

And for 110 lbs. do. do. $= \frac{\cdot 2759}{\text{efficiency}}$.

Here it will be instructive to consider the limit towards which we strive but can never reach. As before, 1 lb. water evaporated from 53 degrees to 100 lbs. absorbs 895,520 foot lbs. of energy. $895{,}520 \times \cdot 2864 = 256{,}477$ foot lbs. are utilised per lb. of water by a perfect engine—an engine expanding to 1 lb. without loss. $\frac{1{,}980{,}000}{256{,}477} = 7 \cdot 72$ lbs. of water are required per I.H.P. per hour by this perfect engine. On the supposition that the theoretical evaporative power of 1 lb. of coal is 13 lbs. of water we have $\frac{7 \cdot 72}{13} = \cdot 6$ lbs. of coal per I.H.P. will suffice for a perfect engine with a perfect boiler. With a boiler of ·6 efficiency $\frac{7 \cdot 72}{8} = \cdot 97$ lbs. of coal will be required for a perfect engine. The difference between ·97 lbs. and the various quantities of coal given in the tables will therefore represent the waste attributable to the engine alone, the waste by the boiler being eliminated.

If the total foot lbs. latent in each lb. of coal be 11,000,000 then $\frac{1{,}980{,}000}{11{,}000{,}000} = \cdot 19$ lbs. are capable of producing a horse-power could the TOTAL heat, latent as well as sensible, be utilised and converted into work. To do this, not only would the steam need to be expanded to 1 lb. without loss during the course of expansion, but before being dispensed with, all the heat still latent in it when at 1 lb. pressure would require to be converted into work.

It has been already mentioned that saturated steam while expanding and performing work was undergoing partial liquefaction.

From the figures with which we have been dealing this can be readily shown, and some important deductions can be drawn from the fact.

Take the engine No. 1, Table II., 100 lbs. for an illustration. In this engine we find that out of every pound of water used 154,397 foot lbs. were transformed into work, as represented by the mean

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pressure. To account for this number of foot lbs. in accordance with the law of the Conservation of Energy, a definite quantity of heat must have disappeared from the steam, and this quantity, moreover, according to Joule's law, will be $= \frac{154,397}{772} = 200$ units of heat. Let us see now whence these units have come.

Total units heat in 1 lb. steam at 100 lbs. = 1213.
Do. do. at $2\frac{1}{2}$ „ = 1153.

The difference = 60 units, which 1 lb. of steam is capable of giving up for conversion into work when expanded from 100 to $2\frac{1}{2}$ lbs. But we have had work done to the amount of 200 units, while here we find that the steam, if it simply expanded, could afford only 60 units. The difference, therefore, of 200 — 60 = 140 units must have been obtained in some other fashion. The only available source is the *latent* heat of the steam, and this can be drawn upon only by liquefaction. Hence it follows that, in this case, 140 units per lb. of water must have been converted into work out of latent heat. Taking the available latent heat of steam at 100 lbs. as being equal to 885, we have the quantity of steam which is liquefied out of every lb. used as $= \frac{140}{885} = \cdot16$ lbs.—*i.e.*, for every lb. of steam which in this engine enters the high pressure cylinder there exists, at the low pressure release, as steam, ·84 lbs., and as water, ·16 lbs. In No. 7 of Table II., where the expansion is only 2·2 times, the amount liquefied is ·07 lbs. per lb. of steam. With no expansion at all, this quantity should disappear altogether; and if No. 8, Table II., be calculated in the above way, a result is got = ·01, which is very near the vanishing point, and which serves to show the correctness of the process.

If any number of cases be worked out, it will be found that the greater the expansion, the greater the liquefaction. An obvious inference from this therefore is, that, of itself, liquefaction in the cylinders is not an evil. It is inseparable from the expansive working of steam, and if, unhappily, the physical laws which govern the action of steam had been different from what they are, then steam

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might have been far less suitable than it is as a medium for transforming energy. Did steam not liquefy somewhat during expansion, our engines would show an efficiency barely half of what they now show. It is in this liquefying tendency that the explanation of the expression $PV^{\frac{10}{9}}$ = constant is to be found. Superheated steam does *not* liquefy because it has a store of heat in the shape of increased temperature, and it can afford to expend this surplus store in work without falling below the least temperature corresponding to a particular pressure. Whenever its surplus store is exhausted, which is very quickly done, it acts as saturated and begins to liquefy.

Liquefaction during expansion is therefore beneficial, inasmuch as it is a vehicle for transforming latent heat into work. Its disadvantage, especially when taking place copiously, is that it aggravates the range of temperature by cooling the cylinder, and by carrying off heat to the condenser owing to its higher conductivity. It is to be remembered, of course, that liquefaction, like expansion, is a loss pure and simple whenever it occurs elsewhere than behind a moving piston. Unless work is being performed, the liquefying of steam merely means that heat is being dissipated.

100 lbs pressure has been taken in these examples as representing a good average of the pressures used in the compound engines of the present day. It is matter of demonstration, however, that higher pressures contain in themselves the elements of increased economy, and the only question is how to manipulate these higher pressures in order to extort this increase. It will be evident that higher pressures mean greater expansion, and greater expansion means greater ranges of temperature, and very early cut-offs. But, with a pressure of 110 lbs., the ranges of temperature, with two expansive cylinders, have been shown to be about 96 and 85 degrees, and the cut-off, in both cylinders, a quarter of the stroke. Were economy alone an object, these would be the adjustments; and to go further than these would be the reverse of economical. But, with a higher pressure than 110 lbs., we *must* go further, else there can be little gain. Hence the origin of the 3-cylinder engine, with triple expansion, to deal with higher pressures. In the very near

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future we are likely to witness a considerable development of this type of engine—a type which, to my mind, presents many worthy features.

To give an example:—Let us assume an absolute pressure of 140 lbs. = 125 lbs. on the boilers, and apply it, first, to a 2-cylinder engine, and, second, to a 3-cylinder engine. For 2 cylinders to attempt to use steam at this pressure, the best proportions might be $r = 3{\cdot}5$, $R_1 = 3{\cdot}5$, $R = 4$, $P_3 = 4$, as before. The receiver pressure, $P_2 = 30$ lbs.; the receiver drop being 15 per cent. The total mean pressure on high pressure = 91·52 lbs. The ranges of temperature being 103 degrees in the high pressure and 97 degrees in the low pressure, with a total expansion of 14 times. The efficiency will be = ·16, and the coal per I.H.P. = 1·7 lbs.

For 3 cylinders the proportions might be thus, viz.:—r in first cylinder = 1·8, r in second cylinder, = 1·9, R_1 in third cylinder = 2, and the ratio of cylinders, 1 : 2·6 : 2·9, or $R = 7{\cdot}54$. The receiver pressures, $P_2 = 53$ and 17. The total mean pressure would be 201·8 lbs. on high pressure. The ranges of temperature in first cylinder = 69 degrees, in second = 65 degrees, in third = 66 degrees, and the total expansion, 13·5 times. The efficiency would probably be ·1843, and the coal per I.H.P. = 1·48 lbs.

The steam is supposed expanded in both engines nearly to the same extent—14 in the one and 13·5 in the other. Excessive ranges of temperature in the 2-cylinder engine, as compared with the exceedingly moderate ranges in the 3-cylinder engine, are met by using different constants. The coal is reckoned on a boiler efficiency = ·6. If this last be compared with the previous examples, it will be seen to consume less coal than any of them at 100 lbs., the nearest to it being Engine 1, Table II., where the coal = 1·6 lbs. This, to some, may seem a small difference in economy due to the 3-cylinder engine; but observe the 2-cylinder engine is cutting off in the high pressure cylinder at $\frac{1}{8}\frac{1}{3}$th part of the stroke, and, in the low pressure at $\frac{1}{4}$th, and expanding the steam 15·2 times without any drop between the cylinders. In the 3-cylinder engine, steam is not cut off in any of the cylinders till half-stroke, and is

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later still in the two first cylinders; and, moreover, in the receivers, a drop of pressure equal to 15 per cent. is allowed. This is therefore an essentially "handy" type of engine. It can be readily started, stopped, and reversed, and would require no expansion valve at all.

In Fig. 11, Plate V., are shown the expanded cards obtained from an engine of this class. The cylinder capacities are in the ratio of 1 : 2·25 : 2·42, or the first is to the last as 1 : 5·44. The ratio of expansion in the first cylinder = 1·47, in the second and third = 1·3.

An inspection of these diagrams as expanded will show that this type of engine is eminently fitted for expansion with the minimum of receiver loss. In fact, with three cylinders there ought to be no toleration of any fall whatever between the cylinders. The principle admits of this very desirable result without necessitating the extremely early cut-offs which are so objectionable in the two-cylinder class.

The practical conclusion to which these figures and considerations point is, in the writer's opinion, towards increase of the use of expansion—*i.e.*, the proportions in the beginning of each of the tables given, are to be preferred to those towards the end. To get the same power, rather larger cylinders would be necessary, and somewhat stronger shafts and piston rods. But the pumps, condensers, and boilers would all be the same as before. In point of fact the boilers would be a little less. The difference in total weight would be inconsiderable if designing were carried out on true principles. The prevailing custom is to make all pumps, &c., in a certain proportion to the cylinders, whatever be the cut-off of the steam or its pressure. The pumps—air, circulating, and feed—as well as the condenser, should be proportioned to deal with the quantity of steam (or water) used, and this quantity depending as it does upon the high pressure cut-off, it is manifest that the latter ought to be a factor in the result.

In the absence of direct and trustworthy experiment theory is helpless. In all that I have here put forward it has been my

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endeavour to build upon experience. In this field, however, as in many another in engineering practice, the dearth of reliable experiment oppresses one. One point in particular might, with much advantage, be made the subject of exact experimental determination. I refer to the relation between extreme expansions of high pressure steam in one cylinder, and the loss of energy occasioned by condensation at the various degrees of expansion. It appears to me that assumption is somewhat readily resorted to in this connection, and that belief in the virtue of passing steam through a succession of cylinders is in danger of becoming of the nature of a faith. The success of the compound engine is undoubted—universal testimony proclaims it. The sources of this success is a different question, and I am of those who believe that too much is attributed to ITS agency, and too little to the surface condenser, and to the increasing efficacy of our boilers, and of high pressure steam.

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TABLE I.

$R = R_1$ (no drop anywhere). Mean pressure = calculated m.p. × ·9.

$P_3 = 4$ lbs. PV = constant.

Superheated steam expanding in well-jacketed cylinders.

$P_1 = 90$.

No.	r	R	M.P.	rR	Max. Loads. H.P.	Max. Loads. L.P.	Temp. Range. H.P.	Temp. Range. L.P.	Eff.	Coal.
1	4·75	4·5	53	21·37	71	67·5	95°	72°	·1941	1·42
2	4	2·16	56	8·64	67·5	40	86°	81°	·1727	1·6
3	3	1·31	59·4	3·93	60	34	70°	97°	·1374	2·01
4	2·46	1	58·8	2·46	53·4	32·6	60°	107°	·1115	2·48
					$P_1 = 100$.					
1	5	4·5	57·9	22·5	80	72	100°	75°	·2088	1·32
2	4	2·04	62·4	8·16	75	43	88°	87°	·1740	1·58
3	2·46	1	65·8	2·46	59·4	36·6	60°	115°	·1128	2·44
					$P_1 = 110$.					
1	5·25	4·6	62·3	24·15	89	78	104°	77°	·2084	1·32
2	4	1·96	68·6	7·84	82	46	89°	92°	·1748	1·57
3	2·48	1	72·6	2·48	55	40	61°	120°	·1146	2·4

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TABLE II.

$R = R_1$ (no drop anywhere). Mean pressure = calculated m.p. × ·9.

$P_3 = 4$ lbs. $PV^{1.9} =$ constant.

Saturated steam expanding in unjacketed cylinders.

$P_1 = 90.$

No.	r	R	M.P.	rR	Max. Load. H.	Max. Load. L.	Temp. Range. H.	Temp. Range. L.	Eff.	Coal.
1	3·6	4	60·05	14·4	68	70	88°	79°	·1668	1·66
2	3·5	3	60·07	10·5	67	55	87°	80°	·1624	1·7
3	3·4	2·5	60·38	8·5	67	47	85°	82°	·1585	1·81
4	3·2	2	60·43	6 4	65	41	82°	85°	·1492	1·85
5	2·8	1·5	61·78	4·2	61	37	73°	94°	·1334	2·07
6	2 2	1	60·75	2·2	52	33	57°	110°	·1031	2·68

$P_1 = 100.$

1	3·8	4	65·07	15·2	77	74	94°	81°	·1724	1·6
2	3·7	3·5	65·88	12·95	76	68	92°	83°	·1700	1·62
3	3·6	3	66·33	10·8	76	60	90°	85°	·1670	1·65
4	3·4	2·5	68·13	8·5	74	54	87°	88°	·1615	1·71
5	3·2	2	67·95	6·4	72	47	83°	92°	·1516	1·82
6	2·8	1·5	69·52	4·2	68	42	74°	101°	·1357	2·03
7	2·2	1	67·95	2·2	58	34	59°	116°	·1042	2·65
8	1	1	86·4	0·					·0602	4·59

$P_1 = 110.$

1	3·9	4	70·83	15·6	86	81	96°	85°	·1762	1·56
2	3·8	3·5	71·91	13·3	85	73	94°	87°	·1741	1·58
3	3·7	3	72·5	11·1	84	65	92°	89°	·1709	1·61
4	3·5	2·5	73 51	8·75	82	58	90°	91°	·1641	1·68
5	3·3	2	74·07	6·6	81	51	84°	97°	·1556	1·77
6	2·9	1·5	74·7	4·35	76	44	77°	104°	·1381	1·99
7	2·4	1	75·06	2·4	68	37	65°	116°	·1147	2·4

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TABLE III.

$R \backsim R_1$ (drop of 15 per cent. to 22 per cent.)

Mean pressure = calculated mean pressure × ·9.

$P_3 = 4$ lbs. $PV^{1.0} =$ constant.

Saturated steam expanding in unjacketed cylinders.

$P_1 = 90.$

No.	r	R_1	R	M.P.	rR	Max. Ld. H.	Max. Ld. L.	Temp. Range. H.	Temp. Range. L.	Eff.	Coal.
1	3	3·5	4	70·3	12	67	74	86°	81°	·1626	1·7
2	2·9	2·6	3	69·81	8·7	66	57	84°	83°	·1561	1·77
3	2·5	2	2·5	76·28	6·25	64	53	79°	88°	·1470	1·88
4	2·3	1·6	2	76·68	4·6	62	47	75°	92°	·1351	2·05

$P_1 = 100.$

1	3·2	3·5	4	75·26	12·8	76	77	92°	83°	·1678	1·64
2	3	3	3·5	79·02	10·5	75	72	88°	87°	·1654	1·67
3	2·9	2·5	3	79·02	8·7	75	63	88°	87°	·1597	1·73
4	2·6	2	2·5	83·2	6·5	73	57	84°	91°	·1508	1·83
5	2·4	1·6	2	83·7	4·8	70	51	79°	96°	·1400	1·97

TABLE IV.

$R \backsim R_1$ (drop of 35 per cent. to 40 per cent.)

Mean pressure = calculated mean pressure × ·9.

$P_3 = 4$ lbs. $PV^{1.0} =$ constant.

Saturated steam expanding in unjacketed cylinders.

$P_1 = 100.$

No.	r	R_1	R	M.P.	rR	Max. Ld. H.	Max. Ld. L.	Temp Range. H.	Temp Range. L.	Eff.	Coal.
1	2·5	2·7	4	92·08	10	76	77	92°	83°	·1604	1·72
2	2·3	2·3	3·5	95·49	8·05	75	73	88°	87°	·1530	1·8
3	2	1·8	3	102·4	6	73	67	85°	90°	·1428	1·93
4	1·8	1·5	2·5	104·2	4·5	70	67	79°	96°	·1307	2·11
5	1·6	1·2	2	106·02	3·2	66	60	70°	105°	·1162	2·38

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TABLE V.

$R = R_1$. $P_2 = \left(\frac{P_1}{r^{1.9}}\right) \times \cdot 88$, which allows a drop in receiver of 12 per cent., in lieu of clearance and wiredrawing; no drop elsewhere.

Mean pressure = calculated mean pressure × ·9.

$P_3 = 4$. $PV^{1.9} =$ constant.

Saturated steam expanding in unjacketed cylinders.

$P_1 = 100$.

No.	r	R_1	R	M.P.	rR	Max. Ld. H.	Max. Ld. L.	Temp. Range. H.	Temp. Range. L.	Eff.	Coal.
1	3·1	4	4	74·07	12·4	75	84	88°	87°	·1601	1·72
2	3	3·5	3·5	75·51	10·5	74	76	86°	89°	·1584	1·74
3	2·9	3	3	76·14	8·7	73	69	84°	91°	·1539	1·8
4	2·8	2·5	2·5	76·5	7·	72	60	82°	93°	·1493	1·85
5	2·6	2	2	76·86	5·2	69	53	77°	98°	·1393	2·
6	2·3	1·5	1·5	77·04	3·45	65	46	69°	106°	·1235	2·23
7	1·8	1	1	74·64	1·8	54	42	53°	122°	·0937	2·95

TABLE VI.

$R \backsim R_1$ $P_2 = \left\{ \frac{P_1}{\left(\frac{R_r}{R_1}\right)^{1.9}} \right\} \times \cdot 88$, which allows a drop in receiver of 12 per cent. in lieu of clearance and wiredrawing, besides a drop of 15 per cent. to 20 per cent. by lateness of low pressure cut off.

Mean pressure = calculated mean pressure × ·9.

$P_3 = 4$. $PV^{1.9} =$ constant.

Saturated steam expanding in unjacketed cylinders.

$P_1 = 100$.

No.	r	R_1	R	M.P.	rR	Max. Ld. H.	Max. Ld. L.	Temp. Range. H.	Temp. Range. L.	Eff.	Coal.
1	2·6	3·5	4	85·68	10·4	74	88	86°	89°	·1553	1·78
2	2·5	3	3·5	87·57	8·75	73	79	84°	91°	·1526	1·81
3	2·4	2·5	3	87·97	7·2	73	69	83°·3	91°·5	·1472	1·88
4	2·2	2	2·5	90·63	5·5	71	62	80°	95°	·1390	2
5	2	1·6	2	91·98	4	68	55	75°	100°	·1282	2·15

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TABLE VII.

$R > R_1$. $P_2 = \left\{ \frac{P_1}{\left(\frac{Rr}{R_1}\right)^{1.9}} \right\} \times .88$, which allows a drop in receiver of 12 per cent. in lieu of clearance and wiredrawing, besides a drop of 35 per cent. to 40 per cent. by lateness of low pressure cut off.

Mean pressure = calculated mean pressure × ·9

$P_3 = 4.$ $PV^{1.9} = \text{constant.}$

Saturated steam expanding in unjacketed cylinders.

$P_1 = 100.$

No.	r	R_1	R	M.P.	rR	Max. Ld. H.	Max. Ld. L.	Temp. Range. H.	Temp. Range. L.	Eff.	Coal.
1	2	2·7	4	103·77	8	74	88	86°	89°	·1446	1·91
2	1·9	2·3	3·5	105·68	6 65	73	80	84°	91°	·1397	1·97
3	1·7	1·8	3	108·9	5·1	72	71	83°	92°	·1290	2·14
4	1·6	1·5	2·5	110·7	4	69	66	77°	98°	·1234	2·24
5	1·4	1·2	2	107·2	2·8	66	60	71°	104°	·1046	2·64

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TABLE VIII.

For calculating mean pressures, for saturated steam.

r	$r^{1.9}$	$\left(\frac{10}{r} - \frac{9}{r^{1.9}}\right)$	r	$r^{1.9}$	$\left(\frac{10}{r} - \frac{9}{r^{1.9}}\right)$
1·2	1·236	·99	3·3	3·77	·643
1·3	1·34	·976	3·4	3·9	·633
1·4	1·45	·938	3·5	4·02	·618
1·5	1·57	·934	3·6	4·15	·609
1·6	1·69	·924	3·7	4·28	·6
1·7	1·8	·882	3·8	4·41	·59
1·8	1·92	·87	3·9	4·53	·577
1·9	2·04	·851	4·0	4·67	·57
2·0	2·16	·833	4·1	4·8	·564
2·1	2·28	·812	4·2	4·93	·555
2·2	2·4	·795	4·3	5·06	·547
2·3	2·52	·777	4·4		
2·4	2·64	·76	4·5	5·32	·53
2·5	2·77	·75	4·6		
2·6	2·89	·732	4·7	5·58	·511
2·7	3·01	·713	4·8		
2·8	3·14	·705	4·9		
2·9	3·26	·687	5·0	5·98	·495
3·0	3·39	·678	5·1		
3·1	3·51	·661	5·2	6·25	·483
3·2	3·64	·652			

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The discussion of this paper was commenced on the 22nd January, 1884, by

Mr J. J. COLEMAN, who said that the author of the paper had, to his mind, omitted a most important element in this question, more particularly as his paper was entitled—"The Compound Engine viewed in its Economical Aspect." Now, he thought it was impossible to speak accurately of the economy of the compound engine unless they took into account the friction in the engine itself. It was commonly assumed that an engine would work with 5 per cent. of its own energy being expended upon itself, and it was well-known that 20 per cent. of the whole force of the engine in waterworks was expended in moving it, in friction overcoming the sliding of the piston rings, and in the friction of the glands That fact was well-known in France where experiments had been made on the locomotive, and where it was found that 35 per cent. was lost upon itself. He had never heard of any experiments having been made to show how much power was lost in the marine engine, and he thought it would be a most important thing to estimate the friction that undoubtedly took place in it. He was of opinion that this friction was increased by turning from the simple to the compound engine, especially when cylinders were horizontal. He had had from 80 to 100 engines passing through his hands during the last few years, used in compressing air to produce cold, and a short time ago his attention had been attracted to this subject. (Explained on black board.) Now, one of the leading lines of steamers had recently got four or five engines from his company, and they insisted on having them of the compound character. He believed that as both of the engines worked horizontally the friction must be greatly increased. He merely wished to point out that fact, and to say that he had a strong impression that the amount of work got out of compound engines was not equal to that obtained from simple engines of the same horse power. In a few months he believed he would be able to give results of the working of four vessels, which he expected would throw some light on this subject.

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M. NORMAND fully concurred with most of Mr Weighton's statements, but he was rather astonished to see that the author of this excellent paper did not seem to entertain a higher appreciation of the essential principles of the compound engine, and expressed such limited confidence in the good reasons which he himself had adduced or recalled. That some of the very worst engines had been produced working on the compound principle was no embarrassing argument. No system, however excellent, could be held responsible for the absurd conditions with which it might be possibly connected. But what was more conclusive, engines had been repeatedly constructed in which all the necessary elements of the compound engine had been brought into play: high pressure, great expansion, surface condensation, steam jacketting, and even superheating. Yet all these combined had been unable to result in marine engines in anything better than three pounds of coal per indicated horse, that is fully 50 per cent. more than in a compound engine otherwise in still the same conditions. The real case of the compound *versus* the single expansion engine is only thoroughly investigated and settled by comparative careful and complete experiments in which all the elements are correctly ascertained, including the most important of all—the true measure of the quantity of heat passing through the engine—which can only be determined by adding to the final or residue heat delivered to the condensing or refrigerating water, that first and most correctly measurable quantity converted into power. The sum of these two (with the unimportant quantity carried away by external cooling) is the measure of the amount of heat delivered to the engine by the boiler. Such experiments in single expansion engines invariably demonstrate that with increased degrees of expansion the real quantity of steam or heat passing through the cylinder is becoming more and more in excess of the apparent or theoretical amount manifested in the indicator curves; this real expenditure of heat is sometimes found to be more than double the useful consumption. Even in compound engines, with the higher degrees of expansion, this increase of expenditure arising from the internal coolings and condensations due to the power realised soon

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puts an end to the advantages of further expansion. It is in such conditions only that the boiler efficiency can appear to stand at so low a figure as 0·6. Thence the reason of the employment of an increased division of the expansion in a greater number of successive actions in three or more cylinders. He (M. Normand) had already constructed a certain number of triple compound engines, and their economy to the extent of 25 per cent., compared with the ordinary compound, had been repeatedly ascertained. Even with the easy and comfortable pressure of 80 lbs. a consumption of only one and a half pounds per horse can be realised.

Mr JOHN TURNBULL, Jun., on being asked by the President to make any remarks, stated that he would gladly do so if the discussion were continued to another meeting. The discussion was thereafter adjourned to next general meeting.

The discussion of this paper was resumed on the 26th February, 1884, when

Mr JOHN TURNBULL, jun., said that at last meeting he had promised to say something when the subject was again brought up. Since then he had gone carefully over Mr Weighton's paper, but he was forced to confess that he had risen from its perusal without having received the satisfaction that he had looked for, or experienced the pleasure or gained the information which he had anticipated. The subject was one of immense importance not only to the shipping world, but also to the industrial and mercantile world, and one of great importance, indeed, to the members of this Institution. It was a subject which he personally had taken a special interest in for very many years past; indeed it was this class of engine which had passed through his hands in the greatest numbers. In his opinion, papers that were brought before an Institution of this kind should either expound some new theory, contain some novelty, or bring before them some important addition to the mechanical details of any machine; but, so far as he could see, none of these were to be found in Mr Weighton's paper. At the outset of the paper an instance was quoted from a report by

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one of the boiler insurance companies, of a compound engine using fuel as high as 6·83 lbs. to the indicated H.P. per hour, from which Mr Weighton deduced that the mere fact of an engine being compound was no criterion of its economy. That he admitted; but he considered that Mr Normand, on the last evening of the discussion, had met that statement in a very satisfactory way when he said that the fact "that some of the very worst engines had been produced working on the compound principle was no embarrassing argument." Neither it was. He himself when writing on this subject some years ago, took the opportunity of laying down this axiom—"When a high state of efficiency had once been obtained in the perfection of any machine, that same state of efficiency could always be attained if the same skill and care were exercised in the latter instance as in the former. Therefore, although there were too many specimens of bad compound engines, that was nothing against the principle. He had met with cases much more astonishing than that quoted by Mr Weighton. In one case he knew of a compound engine taking as much as 12 lbs. of coal per H.P. per hour, but the fault did not lie in the fact of the engine being compound, but was due to the pistons and valves having been allowed to get out of order, to obstructions in the steam passages between the cylinders, and other minor defects, which all taken together led to this disgraceful waste of fuel. In his paper Mr Weighton had laid before them three curves, shown in diagram (Fig. 2, Plate IV.), the first being the curve due to superheated steam in a jacketed cylinder; the second one being due to saturated steam in a jacketed cylinder; and the third due to saturated steam in an unjacketed cylinder; but from these diagrams it was impossible for any one, no matter how desirous of gaining information, to get any idea as to the actual waste that would take place from cylinders clothed or unclothed, or steam superheated or saturated. So far as jacketed cylinders were concerned, he had found from his experience that taking an engine giving out say, 200 H.P., and otherwise well designed, if the cylinders were jacketed the high pressure would give out 90 H.P., and the condensing 110 H.P.; whereas, if the cylinders were

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unjacketed, but still the same proportions and design, the high pressure would give 110 H.P. and the condensing cylinder 90 H.P. This convinced him of the utility of steam jacketing. He understood there was still, especially amongst Clyde engineers, a difference of opinion as to the actual good to be got from jacketing; some were of opinion that the best results were got from jacketing the high pressure cylinder and leaving the condensing cylinder unclothed, and others the reverse. If one cylinder only was to be jacketed his experience was that that cylinder should be the condensing cylinder. Mr Weighton had told them that the secret of economy was to be found in expansive working, that with 50 lbs. pressure an expansion of four times would be about the limit, that is terminating with a pressure of 12½ lbs.; and with 150 lbs. they might expand 10 or 12 times, still terminating with a final pressure into the condenser of 12½ lbs.; but a few pages further on he completely altered this terminal pressure and said: "Let the initial pressure be equal to 100 lbs., and let this be expanded down to 4½ lbs."—a very different thing from 12½ lbs., and one which he (Mr Turnbull) considered much more reasonable and satisfactory. When speaking of that he would like Mr Weighton to explain what he had said on page 73, viz.:—"The diagram (Fig. 2, Plate IV.) exhibits the advantage of working expansively, and a glance serves to show the immense saving which is represented by a difference between four and 12 as ratios of expansion." In this, he thought, there must be some mistake, as the diagram only showed eight expansions. No doubt Mr Weighton could give a satisfactory explanation. "How are we to find the power which, in a proposed engine, will be developed in each cylinder?" was a question proposed by Mr Weighton, but as there would be an occasion later on to refer to this, he would defer his remarks on that point. However, it was stated in the paper that "in the case of a single cylinder engine the computation of the expected mean pressure is a very simple and obvious process, but it becomes, in the case of two cylinders, which are at one time in communication with each other, and which are at another time shut off from such communication, a rather intricate operation."

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With the latter remark he could not agree. For ascertaining the pressure in the single cylinder the formula was $P \frac{1+H}{R}$, P being the absolute pressure, H the hyperbolic logarithm of the number of expansions, and R the number of expansions. For the compound engine the very same formula was necessary, but from that you deduct $P^1 \frac{H}{R-1}$, P^1 being the initial pressure in the low pressure cylinder, and the other letters the same as above. The latter would give the average pressure in the condensing cylinder, and when from the first you deduct the second formula, you get the average pressure in the high pressure cylinder; because he held that this must be understood that for a compound engine to give out equal power to a single cylinder engine the condensing cylinder must be of exactly the same diameter as the cylinder of the single engine. That was a fact which could not be overcome, when they had the same number of expansions in each case. Mr Weighton also gave a formula how to find the mean pressure in the high pressure cylinder, but not by deducting the average pressure in the low pressure cylinder, but minus the terminal pressure in the high pressure cylinder, which is equal to the initial pressure in the low pressure cylinder. He had brought a diagram (see fig. 1) showing that terminal pressure. Mr Weighton stated that you had to deduct this pressure, whereas such was not the case, because the steam when going into the condensing cylinder was going into a much larger area and was gradually decreasing in pressure and enabling more power to be developed in the high pressure cylinder, and less in the condensing cylinder than was shown in Mr Weighton's theoretical diagrams. He exhibited a set of diagrams (see fig. 2) from actual work from a pair of engines working with steam about 120 lbs. pressure above zero, to show the difference in actual practice from Mr Weighton's theoretical diagrams. Mr Weighton had allowed 4 lbs. as the average back pressure in the condenser. This he considered rather high, as there was something wrong mechanically when 12 lbs. vacuum in cylinder could not be got. In regard to Mr Weighton's statement that another inevitable

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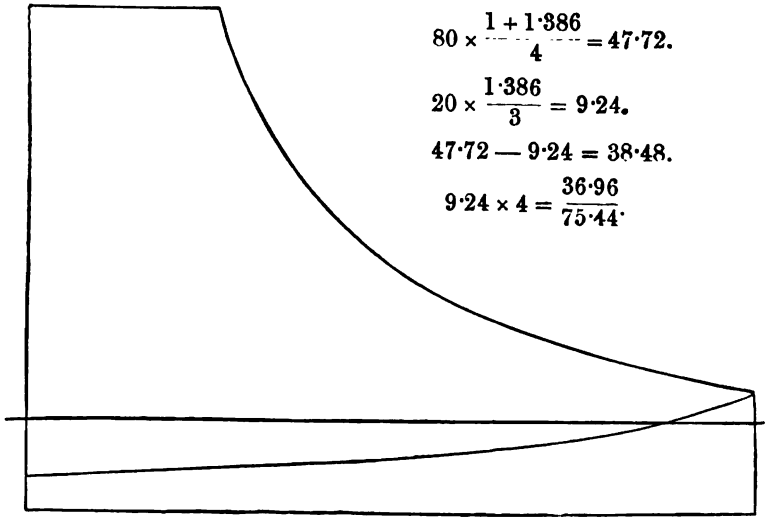
Fig. 1.



$$80 \times \frac{1 + 2{\cdot}77}{16} \times 4 = 75{\cdot}45.$$

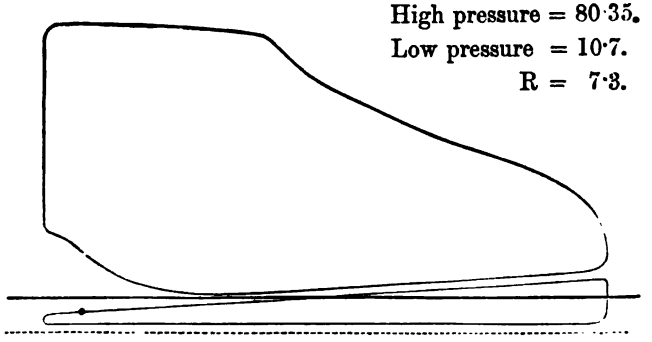
$$80 \times \frac{1 + 1{\cdot}386}{4} = 47{\cdot}72.$$

$$20 \times \frac{1{\cdot}386}{3} = 9{\cdot}24.$$

$$47{\cdot}72 - 9{\cdot}24 = 38{\cdot}48.$$

$$9{\cdot}24 \times 4 = \frac{36{\cdot}96}{75{\cdot}44}.$$

Fig. 2.



High pressure = 80·35.
Low pressure = 10·7.
R = 7·3.

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loss in the engines of the present day was that due to the clearance and ports of cylinders, he thought that too much weight was laid upon the loss in the clearance between the piston and the cylinder ends and the space in the steam ports; at all events, if there was a loss, it could be to a great extent avoided. The set of diagrams shown was not the best case to explain what he meant, but if the slide valve in the high pressure cylinder had been shut a little earlier compression would have begun sooner, and the steam would have been compressed or bottled up until it was possible to reach the same pressure as what was in the steam chest waiting to get into the cylinder as soon as the valve opened, and all that would be lost would be the last stroke or two of the engine when shutting down. It was stated in the paper that equal powers should be developed in each cylinder, and that the rate of expansion in the high pressure cylinder should be equal to the ratio of the cylinders. To make that more distinct he would point out a method by which he arrived at the same conclusion some years ago. It was not a necessity, but it was a desirable thing to aim at, to have equal powers developed in each cylinder, and the method he adopted was to divide the absolute pressure by five and extract the square root. The dividend gave the expansions in high pressure cylinder and the ratios of the cylinders. For to gain as near as possible the maximum of economy he thought they should endeavour to run the steam down to 5 lbs. above zero before it exhausted into the condenser. Assuming the initial pressure to be 80 lbs., divide that by the constant five, and the square root of the dividend would give you R = 4. Thus :—

$$\sqrt{\frac{80}{5}} = 4 = \text{R}.$$

So that according to this formula, with 80 lbs. steam 4 would be the number of expansions in cylinder, and the ratio of the high pressure cylinders. There were some slight matters to be taken into consideration when the rule came to be actually put in practice; but in the case quoted the condensing cylinder would be exactly twice the diameter of the high pressure cylinder, the cut-off would be at $\frac{1}{4}$

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stroke in the high pressure cylinder, and the total number of expansions would be 16. This rule of his (Mr Turnbull) had been adopted by Molesworth in his "Pocket-book of Engineering Formulæ" for the proportions of compound engines, and although not stated as above it was exactly the same rule. Mr Weighton had given them a very elaborate equation at page 80. He wished to refer to the first line of that equation:—

$$P_1 \left(\frac{1 + \log r}{r}\right) - \frac{P_1}{r} = R \left\{\frac{P_1}{r}\left(1 + \log \frac{R}{R}\right)\right\} - P_3.$$

This he found was not the case, for if the example above quoted was taken—*i.e.*, $P_1 = 80$, $r = 4$, $R = 4$—then the one side came to 27·72, and the other 31·72. Of course Mr Weighton had explained further on that the equation was not put in a very convenient form, there being an unknown quantity on each side, but that it could not be further reduced, and was to be used by means of trial and error. In regard to the latter statement, he thought the process of trial and error was undesirable and not necessary, as it was a perfectly easy matter if you knew the pressure of the steam and the power wanted to be developed; and the formula he had already laid before the meeting contained a simple mode of getting at the average pressures. Mr Weighton had been at a great deal of labour in preparing his tables; but, in his opinion, unnecessary labour, because, in each of the tables, cylinders were quoted having ratios of 1 to 1, 1 to 1½, and 1 to 2,—proportions which no engineer of any experience, or who had the most elementary knowledge of the subject, would ever for a moment think of adopting. As a rule, they would find that the want of economy did not arise from the fact of the ratios being much out of proportion, but rather from the causes he had mentioned at the beginning of his remarks. It was stated in the paper that, with respect to the ratios of the cylinders, "there is no fixed relation which is better than any other;" and, a few lines further on, that "there can be only one proportion of cylinders which will give the best result in an economical sense." He could not see how these two statements could be reconciled, as the one seemed quite contra-

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dictory of the other. Mr Weighton also based his formulæ on the assumption that the receiver pressure is constant during the period of low-pressure admission; but in his opinion, that was a most extraordinary statement. It was a perfect impossibility for the steam, after being shut off from the high-pressure cylinder, to do its work in the low-pressure cylinder — expanding into a larger space — without diminishing in pressure. The only way of accomplishing this would be by forcing fresh steam into the receiver space between the cylinders; but this would not conduce to economy. As to having expansion valves on both cylinders of a compound engine, he considered that a desirable thing—the object being to maintain a high initial pressure in the low-pressure cylinder by cutting off early in that cylinder; but he knew of a case where this had been tried with very unsatisfactory results. Both cylinders of a compound engine were fitted with expansion valves; but the engine proved most irregular in its working, as the steam gathered in the space between the cylinders, and, ultimately, a safety valve had to be put on the pipes to allow the surplus to escape. Still, they should endeavour to keep the initial pressure in the condensing cylinder as near as possible to the terminal pressure of high pressure cylinder. In the diagrams from actual practice (Fig. 12, Plate V.), to which Mr Weighton had drawn their attention, they would notice there was a very great loss between the two cylinders—something like 25 lbs.; and, in the high pressure cylinder, the power developed was 1315 H.P.; while, in the low pressure, it was 1670 H.P. This was a state of matters that should not be. The power in the low pressure cylinder was about 25 per cent. more than that in the high pressure; and the difference would have been greater had the vacuum been as it should have been. In this case, it was only 9 lbs., whereas, had it been at 11 lbs.—which amount Mr Weighton desired to adopt, and which was a very reasonable vacuum—there would then have been about 300 H.P. more, raising the power in this one cylinder up to nearly 2000 H.P., while the other was only doing 1315 H.P. In his opinion, that was a compound engine which, from

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an economical point of view, was not getting a fair trial, as it was very much overburdened. It would be found in practice that, as soon as the pressure in the condensing cylinder was raised beyond 5 lbs. above the atmospheric line, they were receding from economy; and they could conceive that this pressure might be so raised as to eliminate the other cylinder altogether, and the result would be that they would be brought back to the single-cylinder engine. Towards the end of his paper, Mr Weighton had remarked that "the practical conclusion to which these figures and considerations point is, in the writer's opinion, towards increase of the use of expansion." Now, while no one would deny the benefit of expansion he pointed out that the economy gained from expansion gradually varies down to a very small limit. For instance, to raise steam from 45 lbs. to 100 lbs., with the same burden, a clear gain of 30 per cent. was the result—or, in other words, you could do 30 per cent. more work with the same fuel; but, when raised from 100 lbs. to 200 lbs., the additional saving was only 19 per cent.; and, again, when raised from 200 lbs. to 300 lbs., the additional saving was reduced to 9 per cent.: and so on, until the saving would be very little indeed, and altogether inadequate to compensate for the loss arising from the leakage of high-pressure steam and the tear and wear of pistons and valves. His experience had not been so much amongst marine engines, which, as a rule, were very carefully attended to. The marine engine seemed to him something like an invalid being sent up the Mediterranean, with his nurse along with him: for there was always a band of experienced engineers ready to attend to the slightest defect which showed itself; and although marine engineers thought that the land engineers had a greater chance of acquiring experience; still, the land engines had not always the nurse at hand, although they might be nearer the hospital. Referring to the equation he had given, he said that he had found in his own practice that they would come near the truth if they deducted 20 per cent. off P, the initial pressure. That would reduce 80 to 64, which, divided by the constant 5, would give 13 nearly, and the square root of that would give a very satisfactory

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proportion of cylinders, and amount of expansion to be carried on through the two cylinders. The using of an automatic cut-off valve had not yet come to be generally adopted in the marine service—indeed, he believed their experience in that direction had been rather grievous; but, undoubtedly, it would be a very important matter if a simple cut-off valve could be introduced, as it also would tend to the economy of steam in the compound engine. In conclusion, he would again say that Mr Weighton's paper had in his opinion added nothing new to the question of "The Compound Engine viewed in its Economical Aspect." Mr Weighton had certainly referred to the advantages of the use of high pressure steam, and the superiority of high ratios of cylinders for high pressures, as well as the economical benefits arising from expansive working, but these were truisms that have now been long known to the profession.

Mr JAMES HOWDEN said that, owing to his not being present when Mr Weighton's paper was read, and having only a few days ago read it over somewhat hurriedly for the first time, he was not therefore in a position to enter into any full discussion of the paper. One did not, however, require to read much of it before discovering it was an excellent paper, and one deserving careful study. Mr Weighton, in part of his investigation on the economical use of steam, approached the subject in the only way in which correct or profitable information can be obtained, and that is by the observation of the effect of the use of the steam through one or more cylinders on the heat of that steam. The two chief points to be studied in effecting economy were the conservation and utilisation of the heat of the steam in its passage through the engine. The conservation was the more important of the two; it indeed included the other; and that engine in which the heat of the steam was most completely preserved would necessarily be the most economical engine. Now, there were two great sources of waste of heat—or he should rather say, two great robbers of the heat of the steam; one was the condenser, and the other the atmosphere. As regards the waste by the condenser, he did not of course refer to the exhaust

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steam, he meant heat from the cylinders, which might have been saved and used in adding to the power of the engine, but which, instead, owing to imperfect design or arrangements, found its way to the condenser, reducing the economy of the engine. The other cause of loss—the atmosphere—carried off a considerable portion of the heat of the steam by its being radiated from the cylinders and other parts of the engine. There is more lost in this way than is perhaps generally supposed. The volume of air passing from the engine room heated up by this radiation represented a considerable amount of power. Mr Weighton had given in his paper, on page 73, an example of waste which might occur in practice, where the metal of a cylinder is first heated up by the working steam, and afterwards subjected to a refrigerating influence. This striking illustration showed how a very large percentage of heat may be wasted in this way. This waste of heat, as he had before said, was in condensing engines carried to the condenser. Now, though the case given by Mr Weighton was a hypothetical one, quite as much could be lost in actual practice. He had, at a meeting of the Institution in 1879 when the discussion on the paper of Mr F. J. Rowan, "On the Use of High Pressure Steam," took place, given an account of a striking case of waste of heat to the condenser, which had come under his observation at an early period of his career as an engineer, and which he might again mention. It was in connection with the steamer "Athanasian," engined on the patent of Messrs Rowan & Horton. These engines were made after those of the "Thetis," which had obtained a great reputation for remarkable economy in fuel. The engines of the "Athanasian" were built expressly to effect a still greater saving in fuel. The "Thetis" had two cylinders, while the "Athanasian" had six, there being three attached to each connecting rod and crank. The three cylinders of each engine were placed vertically across the shaft, the high pressure cylinder in the centre, and the low pressure on either side. The piston rods of each were attached to a crosshead and worked simultaneously, the connecting rod having its upper joint in this crosshead, immediately below the high pressure piston rod. The high pressure cylinder was 9½ inches

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diameter, and the low pressure cylinders each 19 inches diameter, being a ratio of 8 to 1. The steam was intended to be worked at 100 or 110 lbs. pressure, and to be cut-off at half stroke on the high pressure cylinder, so as to expand the volume cut-off 16 times. The design was a great mistake as regards the cylinders. The engines did not prove economical. Before the vessel had run a year her boilers gave way, and he was asked by her owners to supply her with new boilers. Naturally, he wanted to know before designing the boilers, the power at which the engines had been working, but he could get no information, and it was supposed that no diagrams had ever been taken. He, therefore, went to Dublin, and met the vessel on her voyage home from France, and having fitted gear there, took diagrams on the way to Glasgow. The steam being used was then about 90 lbs., being as much as could be got, and as the steam was carried a little beyond the half stroke in the high pressure cylinder, the terminal pressure was on an average about 52 lbs. above the atmosphere. Now, the point he wished them to note was this: On taking the diagrams from the low pressure cylinders, he found the steam, which left the high pressure cylinder 52 lbs. above the atmosphere, had lost the whole of this 52 lbs. pressure by the time it reached the lower pressure pistons, as each diagram began just at the atmospheric line. At first he thought this must have been due to some fault in the indicator, but examination showed it to be actually what took place. As these pistons moved simultaneously there was no receiver required, and the valve box and passages were as small as they could be made. On considering the matter he saw that the whole of the lost heat was absorbed by the metal against which the exhaust steam had to rub on its passage from the high pressure cylinder to the ends and pistons of the low pressure cylinders, and as the ratios of the cylinders were extremely disproportionate, the steam was expanded down nearly to condenser pressure before it was exhausted from the low pressure cylinders. The effect of this was that the metal thus refrigerated in those cylinders and passages was heated up each stroke by the steam from the high pressure cylinder, and the absorption was suf-

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ficient to destroy the heat in the small volume to the extent indicated. As he had previously said, the condenser was the robber of the heat thus abstracted, and the waste in condensing engines was caused chiefly by heat passing improperly to the condenser in this way, and in a lesser degree to the atmosphere by radiation. The study of the question of exact ratios between the high pressure and low pressure cylinders was a small thing in comparison with that of preventing heat from getting to the condenser without performing useful work or of its radiation to the atmosphere. There was one other point in Mr Weighton's paper—the liquefaction of steam—on which he would like to say a few words. Mr Weighton seemed, as he thought, to imply that liquefaction was in itself a benefit, and consequently the greater the liquefaction the greater the economy, as it enabled them to utilise the latent heat of the steam. Without doubt the latent heat in the steam that liquefied, when working in the cylinder, went to support the portion that remained steam, and thus prevented greater liquefaction, but on the other hand it was found by experience that the engines in which the most liquefaction was found were the least economical. For example, single cylinder engines with an early cut-off and high expansion always showed much liquefaction and were not economical. The water of liquefaction itself absorbed a great quantity of the heat, as when the communication between the cylinder and condenser was opened the water was for most part re-evaporated and carried off the heat to the condenser. Mr Weighton, in the concluding remarks of his paper, seemed rather to throw doubt on the compound principle of the engine being the cause of its economy. He could not understand how Mr Weighton should thus throw discredit on the proof which his own paper supplied of the compound principle being the cause of the economy. It was so just because it did not waste the heat, as the single expansion engine did, the low pressure cylinder protecting the high pressure cylinder from the condenser, and the ranges of temperature through which the steam has to work being much less in each. He did not agree with Mr Weighton that the best size for a receiver was $1\frac{1}{2}$ times that of

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the high pressure cylinder. He cared very little what size the receiver was, it did not much matter, but the larger the receiver there was the greater surface of metal in contact with the steam and the greater radiation. Remembering the case of the "Athanasian" he introduced an arrangement of compound marine engine working on two cranks about 17 years ago, where he had carried out the condition as nearly as possible recommended by Mr Weighton of having no drop of pressure between the terminal pressure of the high pressure and the initial of the low pressure cylinder. He had no receiver at all. The steam was admitted to the high pressure cylinder by a simple plate valve, and was exhausted by the valve of the low pressure cylinder, which fitted the space between the adjoining faces of the two cylinders. As the low pressure valves were in two pieces, one at each end, so as to suit ports straight through from the high pressure to the low pressure cylinder, and the steam passed through a port in the valve there was no space to fill with the exhaust steam but the clearance in the low pressure cylinder, and a very short part. The drop in pressure was, therefore, very small. These engines were very economical, but as their cranks required to be placed about 150° or 145° apart, there was considerable difficulty experienced in reversing them after a while, especially with the hand reversing gear then universally used. Owing to their getting into trouble in this way he abandoned this style of engine. It showed, however, that two compound cylinders on two cranks could be worked without any receiver. He had noticed that Mr Coleman, on the opening night of the discussion, had supposed there was much more friction with compound than with simple engines. He did not see grounds for supposing that the friction for same power should be more in the compound engine than in the simple engine. The aggregate surface may be a little more in the compound, but it must be remembered that the large cylinder did not require to be so tightly packed, as the pressure under which it worked was so much less than if the same pressure of steam had been used through two simple cylinders in which both required to be packed for the high initial pressure. He could not agree with the statement that the compound engine

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had necessarily greater friction. Mr Turnbull in his remarks, it appeared to him, had entirely missed the scope and intention of Mr Weighton's paper, and had failed to realise its value. He must also take exception to the statement of Mr Turnbull, that a surface condenser was no better than a jet condenser for producing a vacuum. It was now something like 15 or 16 years since he had read a paper before this Institution on the surface condenser, in which he showed that it was impossible to attain so high a vacuum in a jet as in a surface condenser. This arose from the fact, that in every cubic foot of water there were about 30 cubic inches more or less of air. This entered the condenser with the water jet, and the consequence was that, other things being equal, the vacuum must always be less in a jet condenser.

Mr ARCHIBALD BARR said that, on page 73 of the paper, Mr Weighton made some remarks and adduced some figures with regard to the advantage of compound working, and said that in a single cylinder engine a certain quantity of heat, say the amount required to heat one-sixteenth of an inch thick of the cylinder to the temperature of the steam, was absorbed by the cylinder during admission, and given out during the expansion and exhaust. He would like to know whether Mr Weighton had any actual data on that point, because that was at the root of the difference in economy between the compound and the single engine. Of course, Mr Weighton did not mean that this heating up of one-sixteenth of an inch of the metal of the cylinder was a correct representation of what actually took place, because no sensible thickness of the metal would be heated uniformly. Experiments on that point would be very valuable. He observed that Mr Weighton had given them three-quarters of a cubic foot as the amount of steam required to heat the cylinder for each cubic foot expended in doing the work. He believed this was much in excess of what is actually wasted in most ordinary single engines. If Mr Weighton had any data to go upon for this, he thought these would be valuable to the Institution. There was another point he desired to call attention to. It was on pages 77 and 91 of the paper—"With steam as a medium this would be

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perfection. We would utilise all the sensible heat, and lose only the latent heat." Then on page 91—"It is inseparable from the expansive working of steam, and if unhappily the physical laws which govern the action of steam had been different from what they are, then steam might have been far less suitable than it is as a medium for transforming energy. Did steam not liquefy somewhat during expansion, our engines would show an efficiency barely half of what they now show." Now these passages were apt to be misleading, for according to the laws of thermodynamics, the efficiency of a perfect heat engine was independent of the working fluid, the same efficiency could theoretically be got with steam as with air. He did not think that the latent heat of the steam came into the question at all. Another noticeable point was where Mr Weighton had laid great stress upon having equal powers developed in the two cylinders. What was more important was to have as uniform a drive on the crank shaft as possible. Of course, with any particular engine working expansively, this could only be obtained for one ratio of expansion. He thought that should be looked to more than it usually is. Then with regard to friction, as mentioned by Mr Coleman. He was of opinion that in a compound engine there was a good deal more friction than in a single engine doing the same amount of work. He was of opinion that the formulæ and tables given in the paper would be found very useful to those engaged practically in designing steam engines.

M. NORMAND said that in the great subject brought before them it appeared to him that some most important topics were being completely laid aside. The importance of preventing the condensation of steam during its expansion, as an element of economical working, either in single or double cylinder, or treble cylinder engines, was acknowledged by all, just as that of at first producing dry steam in the boiler. He had known engines and boilers, otherwise perfect, which were rendered almost useless from the single fact that the surface of ebullition was reduced to such a small extent that the rate or speed with which the steam was going through the surface of the water was such as to carry away with the

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steam a great proportion of water. This water implied in itself but a small loss of heat. It might be even said that this heat could be of some useful effect in the expansion; but it was most conclusively settled that a very small percentage of water mixed with steam caused that steam to lose a great proportion of its effect. He might say that in engines working with a great deal of expansion five per cent. of water mixed in the steam would reduce its efficiency by 15 per cent., and that 10 per cent. would reduce it by 40 or 50 per cent. And when they got to 15 or 20 per cent. of water in the steam there was not any more useful effect to be got. This loss of power from the moist state of the steam had been alluded to by Mr Weighton himself in his paper; he had only neglected to draw the conclusions, which were manifestly: to prevent all loss of heat from those deposits of water on the metal of the cylinders, and even upon the surface of the piston itself, as the whole of the surfaces might then be considered as an increased dead space, as chambers of useless expansion or porous matter ready to take away a great part of the steam, which was further given up to the condenser without any useful effect. If they considered that in the low pressure cylinders of the present engine the final volume of the steam at the end of the stroke, in ratio to that of the feed water, was at least as 1 was to 2000, it meant simply that in a cylinder with a stroke of five feet a layer of water of one thirty-second of an inch over the surface of the piston, without considering the other surfaces, would be sufficient to absorb the whole quantity of water represented in the steam, they could understand how easily this effective disappearance of the steam, which Mr Howden had so exactly and strikingly described, could be realised; a stratum of water thinner than a sheet of paper being more than sufficient to represent the whole volume of the disappeared steam. Now, they could fully appreciate the high utility of steam jackets and steam casing, and how they operated in preventing condensation of steam over the envelopes in which it was to be in contact during its expansion. But steam jacketing alone, when applied to engines carrying expansion to a great degree, such as ten times the

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original volume of boiler steam, was quite unable to prevent the whole of this loss in cylinders, and he had no hesitation in saying that the re-heating of the steam, in its intermediate stages, was far superior in efficiency to any amount of jacketing; but what was better still was to have both the re-heating and jacketing as well. Superheating had been used in all countries for years. In 1862, marine engines were built in which the steam was superheated, and many of the steamers plying on the Thames had superheated steam at that time. It seemed, however, that practical difficulties had arisen to prevent it being longer used, and, in most engines, the superheating apparatus had been taken away, and had given place to the compound engine, or even, also, to the old style of engine itself. The true solution was that the two systems should be blended together. It was evident that in most of the modern engines expansion was carried to a further degree than it was really useful. From his own observation, he could say that the ordinary rate of expansion in one cylinder was practically such that one-tenth of the original heat contained in the steam disappeared with that portion which was turned into water. Now, when they had brought the total expansion to eight or ten times, they would have steam mixed up with 15 to 20 per cent. of water. He had seen cases where that expansion was carried to such a damageable extent, in the absence of the necessary accompaniments, that the working was brought back even below the level of the most primitive engine—almost to the state of the atmospheric engine, in which, after all, the principal defect was that enormous condensation took place in the cylinders in contact with the working steam; and these inferior results of the compound engines occurred especially in paddle-wheel engines, or others working slowly. He had known of a paddle steamer in France in which, through an excessive attempt at expansion and economy, the result was such a complete failure and the boat lost so much of its former speed that it was completely wrecked. In order to test the advantages in working of thoroughly dried steam he had made a most conclusive experiment in 1858 on a low pressure marine engine working with steam of about 8 lbs. to the square

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inch. He had simply put, at a point of the main steam pipe, a tubular apparatus heated on one side by steam at 80 lbs. pressure supplied by a small separate boiler. The measurements taken were not only the consumption of coal, but the volume and temperature of the water from the condenser, and also the working marked on the diagrams. The amount of superheating was very slight. They would hardly credit him when he told them that the increase of temperature did not go to more than two or three degrees centigrade, that is five or six degrees Fahrenheit, and yet the change was very great. First, by improvement in the indicator cards, in which the vacuum and other elements were increased; the heat transmitted to the injection water was less. There was on the whole a recognised economy of at least 11 per cent. He had repeatedly applied the same process in compound engines and treble expansion engines. In some cases he could have the steam passing from the first to the second cylinders going directly without being superheated, and, in this comparison, the advantage of the re-heating was most manifest. He admitted with Mr Weighton that they could not have expansion without cooling, since a steam engine was essentially a cold-producing machine; but they were able to undergo this cooling, without its indirect, and, by far, greater damaging effects. The question, though, was not to make new steam out of the condensed parts—that is, transforming the water into steam again, which steam, indeed, at a low pressure, would not be of so much efficiency for power a second time; but it was clearly to advantage that this condensed steam should not be allowed to interfere, without the least reason, with the proper working of the remaining steam, in gaseous state, but that this water should be drained off and sent back to the boiler. The great objection to the superheating in the single expansion engine was that the temperature of the steam, when at high pressure, became too high, whereas this steam was rendered corrosive, caustic, and dry—causing more mischief than it afforded benefit in the use of the steam. But, with the intermediate re-heating in the compound engine, these evils did not arise. A great deal of the restored heat was not used to raise

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the temperature, but simply to dry the steam, which was never brought back to its original temperature. Moreover, the intermediate steam, mixed with moisture and lubricant matter from the cylinders, would not cause the same destructive action to take place in the superheating surfaces. Besides the situation was safer than with the ordinary engine. If any accident took place in the superheating apparatus, it was no more dangerous; an insignificant escape of steam could only result from the measured volumes of steam which even completely ceased at the moment the engine was stopped. In conclusion it was evident that most compound engines as they were yet made were in want of a further improvement to enable them to accomplish a more important amount of useful effect, and that such further improvement could only be realised by an increased expansion, and then the drying and re-heating of the steam at some one or two stages of its work was absolutely necessary.

Mr WEIGHTON said, when called upon to reply, that he did not know whether he would have time that evening to take up the whole of the matters referred to in the discussion. Mr Coleman considered he had missed the point by neglecting the friction of the engine. Unfortunately, abridgement of some of the subjects of his paper had interfered with its consecutiveness in some particulars. His object was not to compare the simple with the compound engine, but to compare different designs of compound engines with each other; and the simple engine was mentioned merely to show that, while compound engines varied in economy, some of them varied to such an extent as to fall below the simple type in efficiency. In compound engines of similar size, the friction would be very nearly constant in them all; but they might differ to a very great extent in their manner of manipulating the steam they respectively received from the boilers; and, if the paper had any point at all, it was to show that economy was influenced very materially by variation in proportions of cylinders, combined with adjustments of valves, in respect of the cut-off points. The end aimed at was stated at the top of page 78—"the expansion of a

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given volume of steam from one pressure to another, without any loss in the process,"—and the proportions best adapted for minimising this loss could be investigated irrespective of friction. He fancied that, for such small engines as are usually employed for freezing purposes on board ship, the adoption of the compound principle would not be advisable, because, in these cases, the friction of the engine bears to the total power indicated a very high ratio, and it was questionable whether the expansion gained by the additional cylinder would do more than compensate for the added friction. M. Normand, and, later on, Mr Howden, had misinterpreted the last sentence of the paper, which had been construed into the expression of a doubt as to the value of the compound engine. What was intended to be conveyed there was, that while the compound engine was an undoubted success, the whole of the economy in the application of steam as a motive power, which had been attained during the past twelve years, could not rightly be attributed to it. There were other sources of economy with which the compound engine was generally associated, such as the surface condenser and high-pressure steam. Besides, it might be possible that a very badly-proportioned compound engine was combined with a very efficient condenser and a very efficient boiler, which produced steam of a very high pressure; and the aggregate result might be something far from contemptible, which, in the absence of inquiry, was very apt to be attributed to the agency of the engine. That an engine was "compound" proved nothing in an economical sense, except that it might be as wasteful as any non-compound specimen. The remarks in which Mr Turnbull had been good enough to indulge showed, either that he had not read the paper attentively, or that he had come to the perusal in a frame of mind the reverse of judicial. Almost the whole of his criticism was the result of grossly misconstruing some parts, and of totally ignoring other parts—so much so, that his (Mr Weighton's) reply must necessarily consist largely of quotations from, and of references to the paper itself. As an introduction, Mr Turnbull had gone out of his way to lay down a general proposition, the adoption of which by

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this Institution would certainly revolutionise its aims and its objects. He had laid it down that no papers ought to be read here except those which promulgated new theories or which laid down new principles. That was to say that all endeavours to improve on the application of well-known theories and principles were to be tabooed! So far as could be judged from his remarks, the paper in question seemed to contain something *new*—at least to Mr Turnbull. That a compound engine might be wasteful through "pistons and valves being allowed to get out of order, obstructions in the passages between the cylinders, and other minor defects," was self evident, but that was nothing to the point, because a simple engine might be wasteful owing to the same causes, and it was the wastefulness peculiar to the compound engine alone, with which the paper avowedly dealt. In connection with the instance of great waste on the first page it was said: "Apart altogether from differences in workmanship, which may explain a deal of the above waste, it may be worth while to inquire into the effect on efficiency of differences in design and proportions." The waste accruing from defective condition or bad workmanship did not come within the scope of the paper at all, it was only that waste—and who would say how much of it there might have been in the above instance—which arose from mal-design. The "utility of steam jacketing" was by no means apparent from the instances adduced by Mr Turnbull. If the only effect of jacketing were to take from one cylinder what it added to the other, it was a wasteful device, all the steam condensed in the jackets being thrown away. Most engineers found that it added to the power of a given engine. A very cursory reading would surely serve to show why the terminal pressure was spoken of at one part as "8 or 10 lbs.," and at another as 4½ lbs. The former values occur in connection with the estimate which is given on pages 73 and 74, of the amount of liquefaction occasioned by an excessive range of temperature in one cylinder, and it is taken so high, simply to give the single cylinder the benefit of any doubt. In the latter case, the 4½ lbs. is brought in to show the theoretical action of an engine, expanding down to as near the back pressure

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as may be (page 78). If the curves (Fig. 2, Plate IV.) did not convey any meaning to Mr Turnbull, perhaps they were not entirely meaningless to others. They were simply graphic representations of abstract mathematical expressions, and, if they did nothing else they served to show how the relation of pressure to volume altered in the three conditions under which steam is generally used. They are, moreover, no theoretical abstractions, but a good average representation of what actually obtains in practice. Than the $\frac{1}{9}^{0}$ curve, for saturated steam in particular, there could be no better average found. Considerations of space alone prevented the diagram from being extended further than eight expansions; but to gain from this an idea of the volume of 12 expansions would entail no very great feat of the imagination, and is compassed by simply adding other four of the equal divisions into which the diagram is divided. Mr Turnbull's misapprehension had arisen chiefly from his non-recognition of the fact that there were other compound engines than those whose cylinders begin and end their strokes simultaneously. These latter were of the old Woolf class, a class now rarely constructed, having been found in most circumstances less economical than the receiver type. And that it was the latter class with which the paper dealt was sufficiently obvious from what was said on page 87 about receivers. The receiver pressure, however, could be calculated for the Woolf engine by the same process as for other engines, the constant used being adapted from experience to suit the altered conditions. This matter, of a mean receiver pressure, had been sufficiently explained on page 75, where there are statements which seem to have escaped Mr Turnbull's notice altogether. There it was expressly admitted that the receiver pressure was *not* constant, but while this was so, it was surely possible to arrive at a *mean* pressure, which, handled in a certain way, would give identical power results with the actual varying receiver pressure. The pressure in a cylinder working expansively was by no means constant, and yet a *mean* pressure was habitually and legitimately used in power calculations. Mr Turnbull considered 4 lbs. as too great an allowance for back pressure. This was a matter of

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individual opinion according to individual experience. The value of P_3 was not a fixed quantity, and he (Mr W.) had chosen 4 because he had found the back pressure as often above, as below this amount. The allowance for clearance was likewise the result of experience. He had found that the cards showed more steam than the valve admissions allowed, and the result of calculation fully bore out his statement that clearance was responsible for this. Compression was very seldom so nicely adjusted in practice as to counteract the action of clearance, but in cases where this was done, the effects would be apparent in the altered values of the constants for calculating P_n as given in an analysis of the actual diagrams. With respect to what Mr Turnbull had said about the alleged "necessity" for equalising the powers, and about the ratio of the cylinders, he would content himself by asking Mr Turnbull to *read the paper*. On pages 82 and 83, in the paragraph beginning "With respect to the ratio of the cylinders, &c.," would be found all the explanation required. And so with regard to Mr Turnbull's astonishment that the steam should be prevented from falling in pressure "while passing from one cylinder to the other." An understanding of the theoretical diagrams previously given, not to speak of the whole drift of the text, would surely show that it was a very essential object indeed to prevent the steam from falling in pressure "while passing from one cylinder to the other." To confound its *action in* the cylinders with its *passage between* them was another proof that it was the Woolf engine alone which occurs to the imagination of Mr Turnbull. In the Woolf engine, where the pistons begin and end their strokes together, the steam may pass directly from the one into the other during the whole stroke (if the low pressure valve carries steam the whole way), and will, of necessity, be expanding and doing work. But, in the receiver type, with the cranks at 90° or thereby, the one cylinder cannot be in communication with the other during the entire stroke, and the exhaust steam from the high-pressure must be stored in a receiver until the low-pressure is ready to admit it. The drop of pressure which is liable to occur between the high-pressure release, and the mean receiver

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pressure, constitutes the fall of pressure, "while passing from one cylinder to the other," which is referred to on page 83. The special reason of Mr Turnbull's objection to the introduction of formulæ was not made at all plain. The simple equation deduced on page 80—and which goes no farther than the veriest elements of algebra—was by no means controverted by the assertion that one side gave one value, and the other side gave another value! This simply served to show that Mr Turnbull failed to appreciate the *meaning* of an equation. The inequality of his result merely proved that he had failed to hit upon the proper values of some one or more of the elements of which the equation consisted ("trial and error" being the only means of solving it). The validity of a general equation like this could be attacked in only two ways; first, by showing the data on which it was founded to be false, or second, by questioning the mathematical accuracy of the method by which it was deduced from these data. On both points Mr Turnbull was silent. Mr Turnbull had said "it was stated in the paper that the rate of expansion in the high pressure cylinder should be equal to the ratio of the cylinders." This was just precisely what was *not* stated in the paper, and was another proof of Mr Turnbull's distorted mental vision in reading it. r of itself had no particular relation to R unless combined with R_1 and P_1, and only in special circumstances would they be equal. (See note at the end, where it is demonstrated from actual practice that equal powers in the two cylinders do *not* follow from the equality of r and R, notwithstanding that this appears to be regarded by Mr Turnbull in the light of an axiom). Mr Turnbull had been unable to understand why in some of the tables the proportions extend to the point where the cylinders become equal (R = 1). On page 81 he will find the reason given in these words: "In order to show the limits within which we can operate I carry out the comparison to the point where the cylinders become equal, and the total expansion then is simply what takes place in the H.P., *i.e.* = r." And again on p. 82: "I do not go higher than 4 : 1 . . . to show the limits of our endeavours." In other words the method adopted being analytical,

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the mutual effects of the various elements (r, R, R_1, &c.) upon each other were best exhibited by carrying the comparison down to unity. The proportions in these tables were not calculated on the supposition that they were all workable, on the other hand those that were feasible were expressly pointed out. (See pp. 81 and 82.) The instance brought forward by Mr Turnbull, where an early cut-off in the low pressure by an expansion valve, caused "steam to gather in the space between the cylinders, and ultimately a safety valve had to be put on, &c.," only served to prove that in that particular case the ratio of the cylinders, or the respective expansions, or both combined were not in their correct proportions for the pressure used. And no doubt the tendency in such a case would be towards irregularity of strain, and of motion. The powers developed by the cards in Fig. 10, Plate V., were unequal, simply because the engines were working at full power. These engines—like most marine engines of any size—were fitted with a variable expansion valve on the high pressure cylinder, and the valves were so adjusted that at the usual working speed the cut-off in the high pressure was such as to produce equal powers in the cylinders, or nearly so. At all other grades than this one, the resulting power would be unequal; later grades would give most power in the low pressure (as in Fig. 10, Plate V.); earlier grades would give most in the high pressure. The rules given on pp. 80 and 82 will show this. The range of temperature though highest in the low pressure is not excessive, and these engines gave a very fair degree of efficiency even at full power. Whether it were the case that in allowing the pressure in the low pressure cylinder to rise "beyond 5 lbs. above the atmospheric line, they were receding from economy," depended altogether upon the initial pressure and on the number of cylinders employed for expansion. If the absolute pressure were 95 lbs., and two cylinders were employed, the initial pressure in the low pressure might be 10 lbs. above the atmosphere without incurring in the low pressure a much greater temperature range than the high pressure. These would be 84° in the high pressure, and 87° in the low pressure. Were the low pressure

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initial pressure kept down to 5 lbs. above the atmosphere, the range of temperature in high pressure = 96°, and in low pressure only 75°. So that with steam of 95 lbs. absolute pressure, it would appear that a maximum pressure of 10 lbs. in the low pressure was a more economical arrangement than 5 lbs. The assertion that no economy resulted from the adoption of the surface condenser in preference to the jet condenser, seemed a strange one, and surely Mr Turnbull's experience had been of a unique kind. The Woolf engine equally with the receiver engine could be treated by the methods given, the only alteration necessary being in the value of the constants, which, of course, were to be got from actual experience. The rule or rules brought forward by Mr Turnbull could not be satisfactory, failing as they did to take account of all the elements involved. The only engine to which they could possibly apply would be to an engine of the Woolf class—and of the Woolf class only—in which the capacity of the intermediate passages was nil, in which the effects of clearance and of wire drawing by the valves were perfectly eliminated, in which the low pressure valve did not cut-off till the extreme end of the stroke, and which was actuated, not by steam but by a perfect gas. Mr Howden had noticed a passage on p. 92 in which liquefaction in the cylinders is said to be beneficial. The liquefaction there referred to was of course that amount of liquefaction which saturated steam necessarily underwent while expanding, and performing work behind a moving piston. This probably was not sufficiently emphasised, but the nature of the calculation on p. 91 showed what was meant. What was said about the limits of size for receivers, refers—as was stated—to engines with cranks at 90° or thereby. Engines like those adduced by Mr Howden having cranks at 160°, and all engines working on cranks between 160° and 180°, did not of course require a "receiver" at all—a mere "passage" was all that was essential. And one of the points intended to be brought out in the paper was, that receiver drop could be obviated otherwise than by adopting this unhandy type of engine. With reference to Mr Barr's inquiry as to data bearing on the condensation resulting

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from extreme expansions in one cylinder, he (Mr Weighton) was sorry to say he had nothing very reliable of a practical kind in his possession. The figures and calculations on pp. 73 and 74 merely stood for what they were worth. They were given in the absence of practical data, and it was the dearth of experiment on this point which was referred to on p. 95. Mr Barr's remarks on efficiency were quite in point, and the efficiency of the perfect engine referred to on p. 20 was found thus:—Efficiency of an engine expanding from 100 lbs. down to 1 lb. pressure without any loss

$$= \frac{(461° + 328°) - (461° + 102°)}{461° + 328°}$$

$$= \quad \cdot 2864$$

But although it was the case that the efficiency of a perfect engine depended solely on the limits of absolute temperature within which it worked, that did not affect the established fact that steam in expanding and doing work partially liquefied. He (Mr Weighton) could not therefore understand how Mr Barr could say that the "Latent heat of the steam did not come into the question at all." Probably he (Mr Weighton) had misunderstood Mr Barr's remark on this point. That the latent heat of expanding steam played a very important part indeed, was shown on page 91. Somewhat more of the principles of thermodynamics applied to the subject would doubtless have been desirable, but he was afraid the result would have been to overload a very practical subject with a deal of theoretical detail. As regarded the desiderated "even drive upon the crank shaft," that of course was impossible with engines working expansively. All that could be done was to see that the maximum loads were not so great as to destroy the bearings, and that the average turning moments were as nearly equal as possible for the two cranks.

On the motion of the PRESIDENT, a hearty vote of thanks was then awarded Mr Weighton for his valuable paper.

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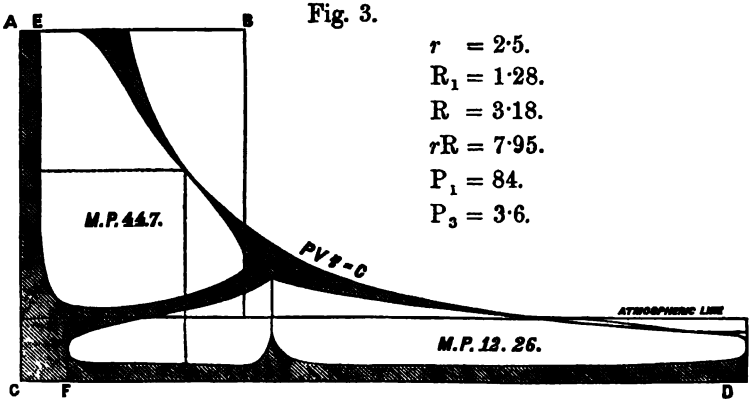
Note received from Mr Weighton, 10th March, 1884.

Diagrams from a Woolf engine were brought forward by Mr Turnbull, and a rule was given professedly applicable not only to the Woolf engine, but to compound engines in general. In the short time at my disposal, at the close of the meeting, it was impossible for me to deal with these as fully as was necessary. I therefore append this note.

The diagram in the annexed cut shows a set of cards which were

Fig. 3.



$r = 2{\cdot}5.$
$R_1 = 1{\cdot}28.$
$R = 3{\cdot}18.$
$rR = 7{\cdot}95.$
$P_1 = 84.$
$P_3 = 3{\cdot}6.$

taken from an engine of the Woolf type. These were compound overhead beam engines, the cylinders being at one end of the beam and the connecting-rod at the other end. The cylinders were 40in. and 72in. diameter respectively, and 10 feet stroke. The valves were of the equilibrium type, and were driven by cams. The high-pressure cylinder top was connected directly to the low-pressure cylinder bottom, and *vice versa*, by means of pipes which were made as small as possible. These intermediate pipes, however, had a capacity which was equal to ·8 of the high-pressure cylinder. This explains the considerable drop of pressure between the cylinders. The cards are expanded thus:—Horizontal distances represent

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volumes; to any scale, E B = high-pressure cylinder volume; A E = high pressure clearance volume; C F = low pressure clearance volume; F D = low pressure cylinder volume; and $\frac{F D}{E B}$ = ratio of cylinders. For purposes of economical comparison, it is essential that the cards be arranged in this expanded form, which shows the variation in volume as well as in pressure. By this process, the curve which the steam has actually followed in expanding from the high-pressure cut-off to the low pressure release is arrived at, and loss by reason of receiver drop is graphically represented. The cylinders were not jacketed, and the actual expansion curve is as near as may be $P V^{\frac{1}{0}} = C$. These engines would be improved in an economical sense by advancing the cut-off point in the low-pressure cylinder. The range of temperature in the high-pressure is 97 degrees, and, in the low-pressure, 89 degrees.

Mr Turnbull's method of computing the relative powers in the cylinders, and hence of deducing correct proportions, is imperfect and misleading for these reasons, viz.:—

1.—It ignores the low-pressure cut-off.

2.—It ignores the receiver or intermediate passages.

3.—It ignores back-pressure in the condenser.

The result of the first omission is that it cannot secure equal powers in the cylinders. In the case of an engine with a given ratio of cylinders, this equality is governed solely by the relative expansions in the two cylinders, and a very small alteration of the low pressure cut-off point is sufficient to materially influence the relation of the powers developed. Moreover, unless the low pressure cylinder carries steam to the very end of its stroke, the rule for the average pressure in the low pressure cylinder is inapplicable; and if steam *is* carried to the end of the stroke, one of the fundamental principles of the compound engine is violated, in respect of the excessive range of temperature which ensues in the high pressure cylinder.

The result of the second omission is, that even in the case of the Woolf engine—to which type alone can the rule of Mr Turnbull be even conceived to be applicable—the *form* of the rule is such that

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it fails to secure the condition on which it is itself based, viz.: no drop of pressure between the cylinders. Note the amount of this drop in the diagram.

As the most convincing of all argument is that drawn from experience, I here adduce an instance from actual practice, which very aptly illustrates the inadequacy of Mr Turnbull's methods. Mr Turnbull says—"It was a desirable thing to aim at to have equal powers developed in each cylinder, and the method he adopted was to divide the absolute pressure by 5 and extract the square root. The dividend gave the expansions in high pressure cylinder and the ratio of the cylinders." Later on he continues—"He had found in his own practice that they would come near the truth if they deducted 20 per cent. off P." Now, it so happens that the following instance practically fulfils the conditions here advocated, with results very different from what is predicted. The particulars are as follow :—

S.S. "Jorge Juan." Earliest expansion grade on trial trip.

$P_1 = 82{\cdot}5$ lbs.

$r = 3{\cdot}79.$

$R = 3{\cdot}73.$

Mean pressures = high pressure = 35·5.
low pressure = 6·46.

Indicated horse power = high pressure = 256.
low pressure = 173.

These are the actual trial results. For a pressure of 82·5 lbs., Mr Turnbull's rule is 82·5 — 20 per cent. = 66 lbs. Then $\sqrt{\frac{66}{5}} = 3{\cdot}64$ = R an l r, according to what Mr Turnbull says above. r and R in the example are 3·79 and 3·73, which is practically in accordance with the rule given. Now Mr Turnbull says these proportions will give equal powers in the cylinders, whereas the actual powers developed were 256 and 173—a difference of 33 per cent.! Moreover the mean pressures as calculated by Mr Turnbull's rules are very far indeed, not only from being *equal* to the developed mean

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pressures, but from having the same *ratio* as the actual mean pressures. Using Mr Turnbull's rules we have :—

$$\text{Total mean pressure} = P\,\frac{1 + H}{R},$$
$$= 82{\cdot}5 \times {\cdot}615,$$
$$= 50{\cdot}7 \text{ lbs.}$$

$$\text{Low pressure mean pressure} = P^1\,\frac{H}{R - 1},$$
$$= \frac{82{\cdot}5}{3{\cdot}79} \times {\cdot}482,$$
$$= 10{\cdot}5 \text{ lbs.}$$

$$\text{High pressure mean pr.} = \text{total mean pr.} - \text{low pressure mean pr.}$$
$$= 50{\cdot}7 - 10{\cdot}5,$$
$$= 40{\cdot}2 \text{ lbs.}$$

That is, the high pressure is 40·2 and the low pressure is 10·5, or the one is to the other as 3·83 to 1. The actual mean pressures are, high pressure 35·5 and low pressure 6·46, or the one is to the other as 5·5 to 1. Comment is needless. The neglect of the low pressure cut-off point must be fatal to all attempts at dealing with the relative mean pressures or the relative powers expected to be developed in a proposed engine.

Note received from Mr John Turnbull, jun., 16th April, 1864.

Copies of the diagrams of the s.s. "Jorge Juan," particulars of which are given by Mr Weighton in his note, have been sent me, and the point of cut-off is shown very distinctly to be at 1-6th of the stroke (making r equal to 6 instead of 3·79); this, with a ratio of 3·73 in the cylinder, makes 22·3 expansions instead of 13·2, as per my rule.

Assuming, on the other hand, r to be equal to 3·79, as stated by Mr Weighton, then the diagrams show distinctly P_1 reduced to 72lbs.

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through wire-drawing, and not 82·5, as Mr Weighton states. Under these conditions, my rule shows 11·5 expansions. The case quoted from actual practice gives 14 expansions.

The diagrams also show a leakage of steam into the high pressure cylinder, and a loss which could be avoided between the cylinders.

Anyone having the diagrams before him can easily corroborate what I have above stated.

Note received from Mr R. L. Weighton, 5th May, 1884.

The first part of the above note may have two interpretations. Its object may be simply to point out that there is wire-drawing at the cut-off points, which every one knows must occur with the ordinary slide valve. If this be what is meant, I have only to add that the cut-off in the H.P. was at 9½ inches in a stroke of 36 inches, that the wire-drawing has the effect of increasing the virtual expansion in the H.P. from 3·79 to 4·5, and not to 6 as alleged above (see diagrams page 138), and that I take the 20 per cent. allowance in Mr Turnbull's rule as covering wire-drawing. But the above note may also be understood as affirming that this wire-drawing is the cause of the inequality of the powers in the two cylinders. If this be the meaning of the writer, the implication of course is that the elimination of wire-drawing would produce equality of the powers—which is the point at issue. By advancing the cut-off in the H.P. cylinder from 9½ inches to 11¼ inches, the influence of wire-drawing—for our purpose —is eliminated; that is to say, the volume of admission of steam at the initial pressure of 82·5lbs. is increased so as to give a *virtual* expansion of 3·73, or very near it (corresponding to the ratio of the cylinders, and therefore fulfilling the conditions of Mr Turnbull's rule, as required in his note). At this cut off of 11¼ inches the mean pressures, calculated on the basis of the actual results, will be about 40·4lbs. in the H.P. and 7·69lbs. in the L.P. cylinder, which still gives a difference in the indicated powers of 29 per cent.

If, as alleged, there be leakage into the H.P. cylinder after cut-off, this difference in power would be increased by a stoppage of such

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leakage. High-pressure valve-leakage, being equivalent to a later cut-off, has the effect of slightly increasing the power in both cylinders, but of increasing it to a greater extent in the L.P. than in the H.P. It follows, therefore, that if there is leakage in this case a stoppage of it would result in a greater disparity than ever in the powers. As matter of fact, however, these cards do not indicate appreciable leakage. Allowing for clearance in ports and cylinder, the expansion curve is as near as may be a pure hyperbola. Neither is there any "loss which could be avoided between the cylinders," except the usual drop of pressure due to the lateness of the L.P. cut-off point, and this drop of pressure the rule of Mr Turnbull is powerless to rectify.

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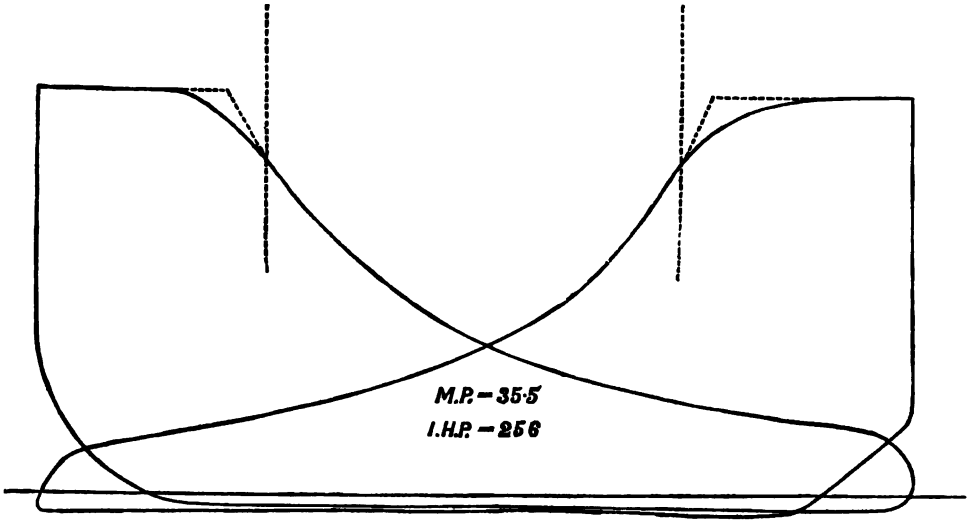
S.S. "JORGE JUAN."

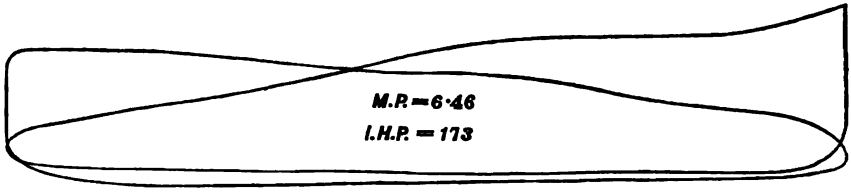


Scale $\frac{1}{32}$.

Steam, . .	71
Vacuum, . .	27¾
Revolutions, .	60

Expansion—cut-off at 9½″

Throttle Valve, full open.



Scale $\frac{1}{16}$.

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On Cable Tramways.

By Mr RALPH MOORE, C.E., H.M. Inspector of Mines.

(SEE PLATES VI., VII., VIII., IX.)

Received and Read 22nd January, 1884.

IN the President's opening address, in speaking of the various new improvements introduced during the last few years, he mentioned, amongst others, the tramway system, which is gradually coming into use in all large cities. One of the most recent developments of working street tramways is the cable system, by which horses are dispensed with in hauling the cars.

In the spring of 1882 the writer had an opportunity of studying the system in San Francisco, where it originated, and he ventures to lay before the members the following short description of it.

Endless ropes for hauling trucks on railways, and for underground haulage in collieries, have long been in use. So little faith was there in the locomotive that, I believe, it was proposed to make a system of rope haulage on the Liverpool and Manchester line in 1830. Better counsels prevailed, and the locomotive was adopted. Until quite recently, however, many miles of rope haulage were in use in the north of England for the purpose of hauling the loaded waggons from the collieries to the shipping port. The London and Blackwall Railway at one time had rope haulage. The Edinburgh and Glasgow tunnel and the Edinburgh and Newhaven tunnel are also examples.

In the three last examples, an endless rope was passed round a pulley at each end of the line, and passing round it to the driving wheel of an engine placed somewhere contiguous, motion was given

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to the rope which ran in the centre of each line of rails. The train was attached to the rope either by clips or by some other arrangement that could be easily attached and detached, and the carriage started or stopped by brakes while the rope continued in motion. In these cases, the rope was on the surface, and was carried by pulleys at the level or slightly above the level of the rails, the height to the axles being sufficient to admit of this.

In America this system of rope haulage, there called the cable system, has been adapted to hauling tramway cars on the street tramways, and it has long since passed the experimental stage.

It originated in San Francisco in 1873 and has been gradually gaining ground in that city, and in other cities in America.

The essential features of the system are a double line of rails, an endless rope running constantly in one direction in a tunnel under the street, which has a slot in the top through which a grip fastened to the car takes hold of the rope. This grip is operated from the car, and when it takes hold of the rope the car is drawn along with it.

The rope is driven by an engine placed at any point of the line. The speed is from 6 to 8 miles per hour, and the cars can be disconnected from the rope, and stopped in as short a space as the present horse cars. The system admits of working gradients which could not be worked by locomotives or horse power, and works with a precision which could scarcely be credited unless it was actually seen.

Its adoption in San Francisco was a necessity owing to the finest portion of the city for residence property being situated on high ground lying immediately behind the business portion, and consisting of hills only accessible by cable roads or by circuitous routes, where it was even difficult for carriages to travel.

Since these cable roads have been introduced, this elevated property has become the most valuable residence property in the city, and has been christened Nob Hill on account of the wealth and value of the property lining its streets.

The Clay Street line was the first where the cable system was

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adopted. This line is a mile long, and the steepest gradient is 1 in 6.

When first projected, it was looked upon by many engineers as very doubtful, and only a few men were got to believe sufficiently in its future to subscribe a small sum for the cost of construction, too small to make a permanent piece of work, and the constructing engineer had to build rather an experimental road in order to gain the confidence of capitalists, knowing that if it was a success, more money could easily be obtained, either to rebuild the line or to keep up expensive repairs. Notwithstanding the manner of its construction the road, which has done and continues to do good service, has amply repaid its proprietors, and has furnished data from which good, substantial, and paying roads have been constructed, not only on hilly routes, but on level roads where horse traffic has its greatest chance of competing.

In San Francisco there are now 6 different companies, owning 20 miles of cable roads running.

The annexed table (see page 148), from the San Francisco *Mining and Scientific Press*, gives the conditions under which they operate.

The most recent line in San Francisco, that of Market Street and its connections, has just been opened and the drawings principally refer to these lines.

The main line extends from the engine house, which is placed at the junction of Market and Valencia Streets, to one terminus at the Ferry a distance of 11,783 feet, and to the other terminus at the end of Valencia Street, a distance of 9,843 feet. There are three other branch lines, two of which, Market Street extension, and Haight Street cable roads are driven by the Market Street engines, and the M'Allister Street, which has its own driving machinery with one terminus at junction with Market Street, where the cars are switched on to main line to the Ferry.

The gradient of Market Street is 1 in 30, and Haight Street, at the steepest part, 1 in 8.

The cars are run every 4 minutes; the average number of cars is 36 to 44, and they run 20 hours and 40 minutes every day. Of

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course, if the traffic required it a greater number of cars could be placed on the line.

The slot, or opening for admitting the grip (A, see Fig. 1, Plate VIII.) is $\frac{3}{4}$ of an inch wide, and is formed by two angle irons, B, running longitudinally the whole length of the line. This opening is $1\frac{1}{2}$ inches to the right of the centre of the track, and the gauge is 4 feet $8\frac{1}{2}$ inches. The rails, C, are flat-bottomed, and weigh 40 lbs. per yard.

The rails and slot iron are carried on what are called "yokes," D. These are made of railroad iron bent to a V shape, with suitable braces of angle iron, &c. They are placed every 3 feet and bedded in concrete, E. The whole space up to the angle iron strut, F, is also filled with concrete, to form a foundation for the causeweying, K, between the rails, leaving the space, G, enclosed by two angle iron struts and the bottom of the yoke, called the tunnel, for the rope to run in.

12 inch pulleys (A, A, see Fig. 1, Plate IX.) for carrying the rope are placed in this tunnel every 25 feet. In the street, over each of these pulleys, is a handhole for cleaning out and oiling the pulley, and cleaning any dirt out of the tunnel.

At each end of the road and at the engine-house are sheaves, A, A, 12 feet in diameter (see Fig. 1, Plate VI.).

The rope is passed round these sheaves and into the engine-house at Valencia Street.

The engines (see Fig. 1, Plate VI.) are two pairs of compound engines, cylinders 24 inches and 34 inches diameter and 4 feet stroke. These engines drive by gearing two main shafts, CC, and are so arranged that one pair or both can work at the same time. Each of these main shafts carries a driving pulley, DD_2, 12 feet in diameter.

The rope coming into the engine-house passes over the front pulley, D, over and under the back one, D_2, round the top of the front one D, and then under the back one D_2, to the stretching pulley, E, round which it passes and goes out again to the rope tunnel, giving a $\frac{3}{4}$ turn round each pulley, DD_2, and this is sufficient

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to draw the load. In this way the rope is kept tight for its whole length.

The rope, which is 3¾ inches in circumference, of steel wire, weighs 2½ lbs. to the running foot. When in its regular position it is in the centre of the track, and at a pulley is 16 inches below the level of the street (see Fig. 1, Plate IX.). It moves at a speed of 8 miles per hour.

In many lines there is a carriage called a "dummy" which carries the grip, and this dummy drags a passenger car after it. This is a very effective arrangement as it admits of easier changing at the termini, and any carriage can be coupled to the dummy.

In the Market Street arrangement the car and dummy are combined (see Fig. 1, Plate IX.). It is 34 feet long and weighs about 4½ tons.

The mode of attaching the carriage to the rope is very simple and effective. Roughly speaking the grip has two jaws in the inside of the tube, which press the rope until the friction is so great that the carriage is drawn along with it. The lower jaw is fixed and the upper one is movable, and is pressed down by some arrangement.

When it is remembered that the slot is only ¾ of an inch wide, it will be seen that the difficulties in the way of getting a compact arrangement are considerable. There are many different forms of grip, but the principle is the same in all. That used on the Market Street line (see Fig. 1, Plate IX.) is a patent of Mr Root, the engineer of the line. A detailed drawing of it is shown on Plate VIII., Fig. 2.

The lower jaw, K, is carried by two arms, I I, which are fixed to a framework fastened to the axles of the carriage, and the upper member, P H P, slides between them, guarded by cross pieces, R, inside the tube and L L above it. The upper member is moved up or down by a lever, A, with a "toggle" joint, D, so that the driver, by depressing or raising the lever, closes or opens the jaws on the rope and sets the car in motion or stops it.

The shank of the grip where it goes through the slot is half-an-inch wide, giving quarter of an inch play.

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The lower jaw, K, carries two pulleys, J J, for the rope running on when the grip is slack, and a plate of soft iron, K; the upper jaw carries only a piece of soft iron or wood, P. The rope is compressed between these two pieces of iron.

This grip enters the tube at the car house, where there is a space left for that purpose, and when once in position it remains there. The lower jaw is placed high enough to clear the pulleys, and when the rope is in the grip it is only $9\frac{1}{4}$ inches below the level of the track (see Fig. 2, Plate VIII.).

When it is desired to stop the car, simultaneously with releasing the rope, the driver applies a powerful brake. On very steep gradients he uses a brake, A (Figs. 3 and 4, Plate IX), which operates on the rails, and by this means the car can be stopped from a speed of eight miles per hour in a space of 10 feet.

Figs. 3 and 4, Plate VIII, show the method of taking hold of the rope. As already stated, the position of the rope, when resting in the lower jaw of the grip, is $6\frac{3}{4}$ inches above the level of the pulley.

In order to bring the grip to the level of the rope there is a depression in the line of rails, between two guide pulleys, so that at an intermediate point the height of the rope and grip are the same. At the same time the grip is moved sideways towards the rope by means of a bend in the rails and slot to the extent of three inches, so that when run in on the line of rope it will take it between the jaws, and the rope will remain there while it and the rails are in the same line.

Where the rope is to be let go, the road has to be lowered in the same way and the rails swerved outwards so that the grip runs out when at the proper level.

At each end of the line, the grip runs out of the rope and the carriage runs on to the turntable, to be placed on the other line on which it runs by gravity, until, by a similar arrangement, the grip lays hold of the rope again.

Where the dummy is separate from the car the turntable may be dispensed with, by letting the car run out of the grip into a single line.

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At the points where the lines diverge from Market Street there are switches as in an ordinary tramway, but in addition there requires to be a switch for the slot so as to prevent the grip from going into the wrong line. This consists of two plates fixed by swivels to the slot iron at one end, and connected by a sliding yoke at their centre. This sliding yoke is connected to the switch lever, and moves along with the points.

When the Haight Street car leaves the Market Street line the rope is let go and the car runs by gravity until it lays hold of the Haight Street rope. It was found that, on some occasions when the points were open for Haight Street the driver forgot to let go his hold of the main line rope. The result was that the grip was broken, and sometimes the rope cut. In order to obviate this, the arrangement shown (see Fig. 1, Plate VII.) was schemed. This consists of two pulleys, A A, which are connected to the switch lever in such a way that when the pulleys are down the switches are open for Haight Street, and when they are up the main line is open. The rope is held down by these pulleys to the height of the guide pulleys, 16 inches below the level of the street, and if the driver has forgotten to let go his hold of the rope, the grip raises it to $9\frac{1}{4}$ inches from the track level, and so forces up the pulleys. This shuts the switches, and the car continues up Market Street. The grip has then to be slackened and the car pushed back to the points.

In order to take the car round the curve from Market Street to Valencia Street, where the gradient is ascending, there is an auxiliary rope which is driven at half the speed of the main rope. (See Fig. 1, Plate VI.) The car coming up Market Street lets go the main rope, and is carried by its own momentum a few feet until it can take hold of the auxiliary rope. This carries it round the curve, and its momentum enables it to run till it takes hold of the rope going along Valencia Street.

It will be seen that there are many ingenious mechanical contrivances needed to keep the rope in position, and also for detachment and attachment of the grip, but these, when properly fixed, work

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with great precision, and a speed of from five to eight miles per hour is maintained.

The cable cars are always preferred to the horse cars. On most routes they go where horse cars could not be run, and then equal speed up hill or down hill is a great advantage. They run equally well on all grades, up or down, or on a level: the weather does not affect them.

These roads, without exception, have been profitable from the beginning; no cable road yet built in San Francisco having been a losing concern. Once in operation they are run more cheaply than when operated by horses, although the first cost of the road is greater.

As regards safety the writer was informed that the system compared favourably with any other system. The San Francisco *Mining and Scientific Press*, writing on this point, says:—

"It was supposed by many that a cable railroad on Market Street thoroughfare would be almost impracticable owing to its crowded state at certain periods of the day. Many thought the cars would have to be moved so slowly that they would be unpopular. Experience has, however, proven these fears to have been unfounded. In fact the line makes better time than any of the cable roads, the cable running about eight miles an hour, and including an average of 30 stops the cars run from one end to the other at the rate of 7½ miles per hour.

"It has been found, too, that the speed at which the cars run tends to make people more cautious than on slower roads. Moreover, the track is kept clear easily. Teamsters know that cars run fast and at the first sound of the gong clear the track, not taking their time about it, as is apt to be the case with the slower roads. People are not apt to run across in front of these cars, but seem rather to prefer letting them pass. The quick speed, therefore, has been advantageous to the road, and is very satisfactory to the public."

The writer sees no reason why the cable system might not be applied to the tramways in Glasgow or other large towns, and be

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more economical and quite as safe as the present system. It might not be necessary to make the yokes of the kind described in the paper. Where the rails are laid in concrete without any sleepers a yoke such as shown by Fig. 3, Plate VIII., might be used. The extra cost of the cable system for three miles of single line would probably be under £5000 per mile. For long lines it would be much less.

With a line of this length the probable cost per car mile would be about 7d.

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TABLE showing Conditions under which the San Francisco Cable Roads operate.

Names of Roads, -	Clay St. Hill R.R.	Sutter St. R.R. Co.	California St. R.R.	Geary St. R.R.	Presidio R.R.	Market St. R.R.
Commenced operating -	Sept. 1st, 1873	January 27th, 1877	April 9th, 1878	Feb. 16th, 1880	Oct 23rd, 1880	Aug. 22nd, 1883
Length of road—double track	5300 feet	On Sutter St., 13,291 ft. On Larkin St., 3712 ft.	12,651 feet	13,200 feet	13,000 feet	44,200 feet
Heaviest grade, - -	67 feet in 412½	36 feet in 412½	75 feet in 412½	40 ft. in 412½	80 ft. in 412½	51 feet in 412½
Number of engines employed,	Two	6; 4 on Larkin St., 2 on Cemetery Aven.	Two	Two	Two	Four—two spare
Weight of empty car, -	2800 lbs.	3000 lbs.	4500 lbs.	4000 lbs.	4300 lbs.	combined,
Weight of empty dummy,	2100 lbs.	2000 lbs.	4100 lbs.	4400 lbs.	4300 lbs.	9600 lbs.
Intervals of departure, -	3 to 5 minutes	4 minutes average	5 minutes average	2½ to 6 min.	4 to 6 minutes	4 minutes
Average number of round trips per day, - -	221	253	226	228	210	438
Average number of cars and dummies employed, -	7 of each	14 of each	14 of each	16 week-days 20 Sundays	15	36 to 44
Hours run per day, -	17½	19½	19	19	17½	20 hours 40 min.
Number of wire ropes in use,	One	Four—Sutter St., 3; Larkin Street, 1	Two	Two	Two	Five
Lengths of ropes used, -	11,000 feet	11,587 ft.; 7849 ft.; 9800 ft.; 8500 ft.	8840 and 17,055 feet	16,600 feet 11,000 feet	12,500 feet 13,500 feet	24,125 ft.; 20,194 ft.; 20,002 ft.; 20,489 ft.; 6000 ft.
Circumference of wire rope,	$3\frac{1}{16}$ inches	3 inches	$4\frac{1}{8}$ and 4 inches	3 inches	$3\frac{1}{4}$ inches	$3\frac{3}{4}$ inches
Speed at which ropes travel,	528 ft. per min.	421 & 786 ft. p. min.	537 ft. per minute	600 & 650 ft. p. m.	537 ft. p. min.	750 ft. per min.
Remarks, - - -	The engine-house is located on top of the hill, and about midway between the two ends.	This company has two engine-houses. In the one there are four engines, in the other two.	The engine-house of this company is located in the hollow, 4220 feet from Kearny Street	The engine-house of this company is situated 8000 ft. from Kearny Street.	Engine-house is 6000 feet from Montgomery Avenue.	The engine-house is 11,900 feet from the ferry.

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In the after discussion,

Mr ARROL said he had great pleasure in corroborating everything contained in the paper. He had had the pleasure of riding on the cable tramways of San Francisco, and he assured them it was remarkably pleasant and steady, and very different from the horse tramways. The starting and stopping were most remarkable, and their easy manner of going up the steepest gradients was noteworthy. He thought the cable tramway system was well adapted for such steep streets here as Renfield Street and Sauchiehall Street.

Mr HILL asked whether there was any difficulty in starting the cars? The speed was 8 miles an hour, so that there must be a tolerably sudden starting.

Mr MOORE answered there was no difficulty in starting: the rope slipped through the grip a little at first, but the car started as quietly as a locomotive-drawn train, and could be stopped as quickly as the horse-drawn cars.

Mr J. TURNBULL, Jun., asked whether the stationary engine which propelled the rope was condensing or non-condensing, and what was the pressure at which it wrought?

Mr MOORE answered that they were condensing engines, and the usual pressure was 100 lbs.

Mr R. DUNDAS remarked that there seemed to be considerable depth from the bottom of the tunnel where the rope worked to the street, it appeared to be about five feet. Could Mr Moore inform them what was the usual depth of the tunnel from the surface of the street?

Mr ALEX. STEVEN asked whether the wire rope was not subject to considerable wear by the action of the stoppage and starting of the cars; and whether the grip itself did not wear out quickly owing to the slip of the rope?

Mr MOORE replied that the bottom of the tunnel was about 3 feet from the causeway. The "grip" must be sufficiently below the angle irons to clear them, and then sufficiently high above the pulleys to clear them. When the "grip" was connected with the rope, the latter was always about six inches above the normal height of the

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top of the pulley. There was a shovel employed for keeping the rope tunnel clear. There was not very much dirt going down into it. The wire-rope was not very quickly worn. The "grip" would not catch on the same place on the rope more than once in a hundred times. They had plenty of experience of the wear of such ropes in collieries.

Mr ARROL asked what was the breadth of the angle-irons in the centre of the track? Many people thought they were objectionable, and must needs cause jolting; but yet it was found that they were not so.

Mr MOORE said the dimensions of the angle irons were mentioned in the paper.

Mr DUNDAS said he understood that when these cars went round a curve, they usually let go one rope and gripped another.

Mr MOORE said that was so; but in such curves as from Renfield Street into Sauchiehall Street the ropes were continued round. In reply to other questions, Mr MOORE said that the wire ropes were made for the work they had to do, and that there was no special danger of the breakage of wire ropes more than of other ropes. With regard to the driving power, all the sheaves were made 12 feet in diameter, and there were $2\frac{3}{4}$ turns, and that was found to be sufficient. In all cases about collieries where there was a three-inch rope working a full power there was no difficulty in one 8 feet sheave to get sufficient adhesion for the rope. He thought it quite unnecessary to have two. He had gone very minutely into all the details, when setting down the cost at 7d per car mile. He believed he could yet add to the paper the information asked—as to how much was spent on the conductors and drivers, and the renewal of the ropes. The cable tramways were, no doubt, cheaper worked than ordinary cars. He could not tell what was the length of the life of a rope.

Mr TURNBULL had heard it stated that the wire rope used at the Queen Street tunnel lasted about 14 months, and cost £600. He did not know whether that was correct.

The PRESIDENT said that as far as he could gather, the wire rope

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in Queen Street tunnel sometimes lasted two years, and at other times it had broken after nine months running, and that was due to the bad quality of the wire rather than the nature of its work. He believed they might assume reasonably that a rope would last from 18 months to 2 years. With regard to the seizing of the rope by the "grip," in a line of some 10,000 yards there would be several cars all gripping at the same time, which would strain the rope considerably, and endanger its breaking. He did not know whether the length of time these cable tramways had been in use was sufficient to enable them to arrive correctly at their actual cost for maintenance. Mr Arrol had remarked on the smoothness of travelling by these tramways, and compared it with that on the Glasgow streets. He thought that was somewhat to be accounted for from the absence of great traffic along the streets. Unfortunately for the tramways of Glasgow the gauge of the car-wheels was the same as most of the ordinary vehicles; and hence it was always the aim of lorry and other drivers to get their carriages on to the tramway rails, which got quickly worn down, and hence much of the jolting was caused by our immense street traffic.

Mr MOORE said that these tramways had been running a sufficient time to enable them to judge of the duration of the ropes. He believed the ropes wore about 18 months, which he considered was very good work. In collieries they considered from 12 to 18 months good wear. He did not expect that the life of any rope would be 18 months. His calculations were based on the rope only lasting one year.

The discussion was then adjourned to next general meeting.

The discussion of this paper was resumed on the 25th March, 1884.

M. NORMAND asked permission to make a few remarks on the conditions of safety and durability of the wire ropes applied to hauling purposes which formed an important part of the subject involved in the most able and interesting paper read by Mr Moore. He had lived at Lyons in France, where existed two remarkable specimens of tramways drawn by wire ropes. The older of the

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two had been working for about 30 years, and connected the main part of the city with the plateau of "la Croix Rousse." The length of the line was about one third of a mile, and rising 400 feet. The gradient was about 20 per cent. The line was straight, and for the main part went under a tunnel. There were two sets of rails, upon each of which moved a small train of passenger carriages, and a series of trucks, the upper part of which was disposed in a horizontal platform, where ordinary trucks or lorries with their load, horses, and drivers took place, to be drawn with the rest of the train by one end of the wire rope propelled by a fixed engine at the head of the incline, one train going down at the time the other was being drawn up. The ropes were made of steel wire, and although they were replaced every year, yet towards the end of their period of service they looked as if they were made up of small lengths of wire, the broken ends bristling out in every direction; and in fact it was found that they were all broken into lengths of 12 or 15 feet. Another such tramway has been erected on the same principle, but under still more awkward circumstances. It connects the lower part of the city of Lyons with the plateau of St. Just, about 500 feet high, and the inclination in that case is about 25 per cent. of the horizontal distance travelled. There is no other traffic on it but passengers and luggage, yet the wear of the wire ropes was faster than on the other line. He had also been connected with some applications of wire ropes to towing in Belgium. They had been tried also on the Rhine and on the Rhone, and there likewise on examination he found them to come very soon into bad condition, and a quantity of the wires broken. He would finally mention one of the first applications of wire rope to haulage 40 years ago on the London and Blackwall railway, and there also the rope could not be kept in good condition, plenty of the wires being broken and sticking out. This rapid destruction of the wire ropes would at first seem most extraordinary, considering that the diameters of the winding pulleys generally bore such a proportion to the diameter of the wires in the rope that no extension beyond the limit point of elasticity of the metal would appear to take place. As an example,

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the diameter of the wires being $\frac{1}{10}$ of an inch, and the diameter of the winding pulleys being 100 inches, the elongation or stretching of the metal on the extreme edge of the half circles outside of the centre or neutral axis of the wire was kept within $\frac{1}{1000}$ of the length, which would be admitted as a very safe condition. But, on a thorough examination, the case would appear quite different. First of all, he would make this general observation, that in this case they were not dealing with a wire in a state of rest. It was a wire that was already stretched, if not to its absolute, ultimate extent, at least very near to its extreme practical capability, so that two unfavourable conditions would ensue—1, that the elongation due to the bending of the wire over the circumference of the drum or pulley would be added to the stretching already resulting from the regular working strain; 2, that the strain distributed over the section of the wire could no more be kept uniform, but that the outer portion would be brought to bear its extreme measure of strain when the inside part would sustain a very limited part of the load; in other words, a most incomplete utilisation of the resisting capacity of the material would be realised. These were yet only the theoretical and therefore the most favourable conditions of working that could be considered, but, in practice, there were several unfavourable circumstances against which they had to provide. The bearing pulleys, although dealing with inconsiderable efforts, were often from their wrong dispositions, the cause of another damaging action on the wires. Generally the sustaining pulleys were restricted to small diameters, therefore, if set apart at such distances that the wire rope should be compelled to conform itself to the contour of such small circumferences, another stretching action of greater intensity would take place in the wires. It will be easily seen that it is of no consequence whether this application or conformation of the wire rope to the pulley should take place through a longer or shorter portion of its circumference, through an arc of 30 degrees or 3 degrees only, so far as any real adaptation is to be effected involving the stretching action on the outside of the wires in the stated proportion of the respective diameters of the wires and pulleys. It thus appears desirable

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that in all cases where the sustaining pulleys cannot be made to the great diameters of the winding drums, they should then be established at such short intervals that the wire rope could be sustained by its tangential contact without involving any real adaptation to the contour. The employment of winding drums not accurately turned would also introduce elements of damage from the inequalities of the contact surfaces, parts of which then represent arcs of circles of much reduced diameters. At last they had to consider that, over all the conditions just mentioned, they had not to deal with isolated wires passing from the straight line of their free stretched state to the circumference of the drawing or bearing pulleys, another source of damage arose from the fact that the wires being twisted in a coil, or still worse, in a cable, the total pressing strain due to this mass was borne not only by the exterior wires alone, but over a small proportion of the extent of these. The following conclusions manifestly imposed themselves:—1, That the winding and sustaining pulleys should be made of as large diameters as possible, that is, generally much larger than most employed at present. 2, That, in the case of small bearing pulleys having to be resorted to, their number should be such that no deformation of the rope at contact could take place. 3rd, That the bearing surfaces of the winding and sustaining pulleys should be accurately turned and the section of the groove closely adapted to that of the rope, so as to have the contact extended to as great a proportion as possible of the wires. 4th, That in no case should the coils of wire rope be wound up over another layer of the same wire rope, as most destructive strains would then ensue. He hoped that the great end of increased immunity from danger to human life, which was the chief object of the above remarks, would be deemed a sufficient excuse for taxing to such an extent the kind attention of this Institution.

Mr C. P. Hogg said as one having something to do with tramways he felt much indebted to Mr Moore for his paper on cable tramways, and especially for the illustrations, which had given him a very much clearer idea of the subject than any mere description would have done. Whatever difference of opinion there might be with

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regard to the use of cable tramways on ordinary routes, there was no doubt but that system was specially applicable for steep gradients, such as existed on the San Francisco line, in Edinburgh, Dundee, and other towns. In the former discussion of the paper, Mr Arrol had remarked that riding on cable tramways was remarkably pleasant and steady, and very different in those respects from the horse tramways. He wished to point out that the mode of traction had very little to do with this, and that the mode of construction had much more to do with it. For instance, it was well known that the tramways, in which the rails were laid on timber, were much smoother to travel on than where the girders were laid on the concrete. Of course, it also lay in the jointing of the rails, for where that was badly done the travelling was very unpleasant. It, no doubt, also depended greatly upon the condition of the rolling stock. Every one knew the annoyance of travelling in a car with jingling windows and bad springs, resulting in much jolting and noise; whereas, cars with super-springs and fixed windows were found to be very different. Of course, the cars of San Francisco may be in good order; but there can be no question that however pleasant travelling by cable might be, it must be very different from riding in one's own carriage, as the tramways are laid with the American step rail. He would like to know if Mr Moore had any figures to inform them as to what power it took to run a line of cable of a certain length, eight miles an hour without the cars; and also whether 7d per tram mile was the whole cost of working or merely the haulage only. On some of the best lines in this country the horse haulage was done for 5d, and with the traffic expenses the total cost came up to 8d or 9d.

Mr THOMAS D. WEIR regretted that this interesting paper had not been supplemented by fuller details of the cost and working expenses. There could be no doubt that by the cable system tramways could be successfully worked in situations impracticable by ordinary means of traction; but it could not be considered as proved that it would be more economical than horse or ordinary steam haulage where the gradients would permit of these being adopted. Mr Moore stated the working cost at 7d per car mile, but did not mention whether this

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included more than the mere expense of haulage. In Glasgow the total working cost, including traffic expenses, taxes, &c., was about 9½d per car mile, of which 6¾d was for horsing alone. On a short English line, worked entirely by steam, the total working cost was a fraction over 7¾d per car mile. In comparing these with the cost of the cable system, an important item was the interest on the additional first cost of the latter. Taking Mr Moore's figures for this, and assuming that the saving in working expenses might amount to as much as twopence per car mile, it would be seen that an annual traffic of not less than 60,000 car miles per mile of tramway would be required before the saving effected would pay the interest on the additional capital outlay. Out of upwards of 130 tramways in operation in the United Kingdom, in 1882, only some 20 commanded a traffic of this extent.

The PRESIDENT said that at the previous meeting a question had been asked with regard to the life of the wire rope upon the North British Railway in the Queen Street tunnel and incline. At that time he had answered the question from memory—that sometimes the wire rope had lasted from 18 months to two years. He would supplement his answer now from information he had since received. Up to 1855 the number of trains passing daily was 20, averaging nine carriages and capable of taking 214 passengers. The gross weight of each train was about 100 tons, the average weight of train for each passenger carried being about nine cwt. The wire rope lasted at that time about two years. Up to 1866 the number of trains daily running was 35, and the rope then lasted on an average 18 months. Up to 1884 the number of trains daily had increased to 70, with a gross weight of about 170 tons, and capable of carrying 420 passengers averaging eight cwt. per passenger, and the rope lasting a little over 12 months. The shorter life of the rope now was easily accounted for, from the fact of the trains being very much heavier and the passengers more numerous, and therefore the rope had to do more work now than formerly. Besides, with each train now they had a surplus of brake power, having a weight of one ton to every ten tons of train that passes through

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the tunnel. That corroborated to a certain extent what he had stated the other night. He thought Mr Hogg had come to a very sound conclusion as to some of the recommendations of the cable tramways; such as their use for heavy traffic, and for steep inclines, where horses would have a difficulty in getting up. He thought such were very suitable places to put down cable tramways, which undoubtedly in their first cost were very expensive; and where the traffic was heavy it would be a great advantage. Then again, Mr Hogg referred to the jolting of the cars, which doubtless depended more on the length of wheel base and the build of the cars than on the haulage. If the cars were seated on bogies similar to railway carriages they would be much easier for riding in.

Mr MOORE said the President had taken up the point of the wear of the wire rope very effectively, and had shown that twelve months was the present life of the Queen Street incline wire rope. It was found that in collieries they could not depend on a wire rope for a much longer period of wear than that. In San Francisco they had a very simple way of testing the wear of a rope. In each engine-room the wire rope passed through a gauge, to which a bell was attached; if there were any obstruction, the bell rang, and thus drew attention to any of the wires being out of order. He was afraid M. Normand was mistaken with regard to the thickness of wires in ropes: there were none one-eighth part of an inch now-a-days. Steel wire ropes were very fine, and went round curves very well. There was no difficulty whatever in the wear of ropes in cable tramways any more than any other. The more weight carried the more wear there will be on the rope. At the same time he did not think that the wear was in exact proportion to the weight drawn M. Normand had not told them whether there was a tube in the French cable tramways.

M. NORMAND answered that there was none.

Mr MOORE said that 95 per cent. of colliery ropes in this country were now made of wire, and it was found they did not break—that some years they raised 150 million tons of coal in a year and had not a rope broken. He thought that a rope one inch in diameter

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should have a pulley of eight feet in diameter to run over; and if the rope was thicker the pulley should be ten feet.

M. NORMAND said that in France and in Belgium the mines were generally deeper than in this country, and there the diameter of the winding pulleys was generally 20 feet, and of the return pulleys over the mine ten feet.

Mr MOORE said with regard to the cable tramways that there was no great weight upon the rails, that in the construction of the roadway it was so well tied that the gauge was very well kept. The 7d per tram mile he had stated he had meant to refer to street tramways in Scotland, and it included all charges, such as interest, and indeed Parliamentary charges. It would easily be seen that where the traffic was great the cable system must be a very inexpensive mode of haulage, at least as compared with horses. The great matter was, that whatever the gradient of the line was, there was the same speed kept up, and also that they could put on any number of cars. He did not know that Mr Weir was right in his surmise that cable tramways would not pay, as it was well known that where facilities for traffic were given, it always increased. With regard to the level roads, he did not agreee with him; for he believed that cable tramways would be worked very much cheaper, and quite under the figure that he (Mr Moore) had given them.

The PRESIDENT then moved a hearty vote of thanks to Mr Moore for his paper. He said that the working of cable tramways was one of the subjects which was beginning to agitate men's minds, and if the particular tramway mentioned in the paper had been so successful as Mr Moore had demonstrated it to be, and could be constructed and worked at 7d per mile in this country, he thought it was likely before long to be introduced in some parts of this city.

The vote was unanimously awarded.

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On the Reliability of Tests of Turbines.

By Professor R. H. THURSTON, C.E., &c., Steven's Institute, New York.

Received 22nd January; Read 25th March, 1884.

THE writer has been reading with interest the paper of Mr John Turnbull, Jr., and the debate "on Water Wheels and Turbines," published in Vol. xxvi. of the Transactions of the Institution of Engineers and Shipbuilders in Scotland, 1882-83. In the discussion of the paper, some questions are asked relative to the construction and principles of operation of the wheel, which the writer may be able to answer; and some doubt is intimated as to the accuracy of the apparatus and the method of test, which I am in a position to meet. The builders of the wheel which Mr Turnbull describes, having asked the writer to make an examination of the flume, of the methods of test, and of the details of calculations from the data taken at the flume, and a statement regarding the reliability and accuracy of the work done there, and of the efficiencies reported by the engineer of the Water Power Company, the writer was accordingly requested to make such an examination and report.

In explanation of several matters, in regard to which inquiry was made in the debate reported in the Transactions, the writer would say: The Hercules wheel is, he judges from its peculiar performance, for he has not had time, as yet, to work up its elements as he hopes to do later, a wheel "of free deviation," as the French engineers call it. The water enters the wheel through a set of straight, or nearly straight, guides, flows first radially, as in the Thomson wheel, and then downward, the wheel thus falling into the class of "inward and downward" flow turbines which have become a most common form of turbine in this country. Nearly all of the most successful wheels are now of this class. Their maximum efficiencies are usually very

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high at full gate—80 to 90 per cent.—and they differ principally in the loss of efficiency with closing gate. The wheel here referred to seems to be peculiar in two respects: it has maximum efficiency at a little less than full gate, and it holds a very high efficiency until the gate is well closed, and the power of the wheel is greatly reduced. The writer is inclined to think that the first of these peculiarities is due to some defect in proportion of admission area to area for discharge. The turbine is invented, and is proportioned and designed, by a mechanic of the true "Yankee type" who, without education of the slightest knowledge of the theory of hydraulics, has made a remarkably good wheel by pursuing simply the intuitions of the natural mechanic. He works out his bucket patterns with his own hands, and neither he nor anyone else has ever yet made a drawing of them. Very probably had he been educated he would have lost the acuteness of perception in this direction, which now distinguishes him.

The wheel being a turbine of free deviation, the water enters it at the velocity due the effective head, and this velocity remains unchanged with the closing of the gate, which shuts down from above, closing off the gate openings, without altering the speed of best efficiency. The speed of the wheel is finally adjusted, for the several heads, by test at the flume, and thus no loss by whirl can occur and maximum efficiency is insured. This efficiency is also retained with closure of the gate until the friction-losses become seriously large as a proportion of the total power of the turbine. The writer thinks it possible that the loss of effect seen at full gate, as compared with 7-8ths gate, may be due to the conversion of the wheel at full-gate, into a "pressure wheel," thus producing a change in the best speed to which the wheel is not adapted.

The tests here referred to were made at the new testing-flume, erected by the Holyoke Water Power Company, for the purpose of testing wheels to be used in mills drawing water from the canals of that company, and with the object, on the part of the company, of determining their consumption of water, and of making the gate a meter for the wheel, in order that the engineer of the company may

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be able to report the amount of water used to the office in which the bills are made out against consumers. The flume is under the engineer of the company, and the work is done under his direction in all cases, and by his assistants. The flume is large, remarkably well designed, and equally well constructed. The weir is 20 feet wide, and the water is measured by the use of the hook gauge. The flow is determined by the standard Francis formula. The writer trusts that these particulars will prove interesting to the members who took part in the debate on the paper of Mr Turnbull, and to all who are directly or indirectly interested in this department of engineering.

It should be added that, when the wheel is tested, it is set in the bottom of the penstock, and the head of water above it is thus unreduced by friction of supply-pipes, and the wheel is subject only to losses due to its own construction and operation. The several tests are made at each stage of "gate" for the purpose of determining the best speed of wheel.

The flume at Holyoke, having ample capacity for all tests ever likely to be desired, being planned and constructed with the greatest care, and with that knowledge of the necesssities of the case which has been the outcome of long experience in testing turbines, being under the control of a body having no interests to affect these of makers unfavourably, and placed, as it is, at the service of the public without discrimination or reservation, may well be made the standard for the whole country.

Recurring to the performance of the Hercules wheel, as tested at the flume of the Holyoke Water Power Co., it is found that its work is remarkably good and remarkably uniform. It exhibits none of that variation in the power and efficiency under apparently similar conditions which has been so common a feature of the tests of turbines hitherto. Its power of doing work with a restricted supply of water, and with little loss of efficiency is something unusual and something to be especially remarked as a valuable characteristic of the wheel. There are presented, in this connection, the following figures. In studying them, it is to be carefully kept in mind that

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the proportion of gate-opening is not always, in turbines, a measure of the quantity of water flowing through the wheel. With some wheels, half gate means three-fourths the full flow; and the statement of gate opening may be quite deceptive. In the Hercules wheel, as tested in the writer's presence, and under his inspection, as is here seen, half gate means about six-tenths full flow, or four-tenths gate means about half full flow; and the efficiency falls but little as the gate closes and as the quantity of water supplied falls off, until the gate is nearly half closed. Thus we have, for the three tests of the same wheels made as above described, at different dates;—

Prop'tional part of Gate opening.	Prop'tional part of Water used.	H.P. given.	Effic. obtained.	Date of Test.
1·000	1.000	146·11	85·24	Aug. 13th, 1883.
·800	·875	133·91	86·94	
·645	·752	115·75	85·85	
·490	·608	88·78	79·89	
·390	·497	68·83	74·66	
1·000	1·000	147·15	84·75	Oct. 10th, 1883.
·796	·874	133·56	86·18	
·654	·755	116·44	85·36	
·489	·600	86·25	78·16	
·382	·500	66·95	72·28	
1·000	1·000	145·72	85·80	Oct. 13th, 1883.
·806	·884	130·28	87·08	
·647	·749	111·65	86·33	
·489	·606	84·79	80·05	
·379	·494	65·06	73·06	

The quantity of water flowing at half gate, with different makes of wheels, varies from figures like those above given, to, not infrequently, two-thirds, and sometimes to three-fourths, of full flow, as has been stated already; the efficiency usually falls off correspondingly. Rankine gives the following as average figures, and beside them are placed the corresponding figures obtained by actual test of

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a wheel doing good work at full gate, and those obtained by test of the Hercules wheel.

Rankine's proportions:	Opening of gate,	0·400	0·500	1·00
	Effic. Max.	0·57	0·72	0·86
Sample wheel:	Gate opening,	0·400	0·500	1·00
	Effic. Actual,	0·60	0·66	0·86
Hercules wheel:	Gate opening,	0·400	0·500	1·00
	Effic. Actual,	0·746	0·799	0·86

The wheel with which the Hercules is compared, does about as well as the highest authorities would expect, and the Hercules wheel excels it, nevertheless, by about twenty-five per cent.

The discussion of this paper took place on the 22nd April, 1884.

Professor JAMES THOMSON said he would not enter on any lengthened discussion of the paper, but he would call attention to one passage in it which he thought did not tend towards a favourable judgment :—"The turbine is invented, and is proportioned and designed, by a mechanic of the true 'Yankee type,' who, without education of the slightest knowledge of the theory of hydraulics, has made a remarkably good wheel by pursuing simply the intuitions of the natural mechanic. He works out his bucket patterns with his own hands, and neither he nor anyone else has ever yet made a drawing of them. Very probably had he been educated he would have lost the acuteness of perception in this direction which now distinguishes him."

Mr JOHN TURNBULL, Jun., said that was certainly a passage which asked attention. Dr Thomson must have forgotten that Sir William Herschel had no special education to help him to his discoveries, and that he produced his great telescopes simply by his own natural intuitions. The paper itself was one dealing in a large degree with a paper which he had the honour of bringing before the Institution during last session; and certainly it was a paper very gratifying to him. Professor Thurston was a gentleman he knew not, and whom he had never met; and, therefore, his testimony to statements he (Mr Turnbull) had made, and which

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were met by many shakings of the head, both by his learned friend Dr Thomson and Mr Richard Murray, was most welcome. In regard to the tests which he had brought before the meeting, a good deal had been said about their not being very reliable—that those tests had been conducted by a neutral body, not for any particular firm of turbine makers, and Professor Thurston had repeated almost word for word what he (Mr Turnbull) had said:—"The flume at Holyoke, having ample capacity for all tests ever likely to be desired, being planned and constructed with the greatest care, and with that knowledge of the necessities of the case which has been the outcome of long experience in testing turbines, being under the control of a body having no interests to affect those of makers unfavourably, and placed, as it is, at the service of the public without discrimination or reservation, may well be made the standard for the whole country." This American turbine, namely, the "Hercules," was highly recommended, not because it was the best turbine with full water, but because its virtues came into play with dry seasons—giving high results with a diminished supply of water. Professor Thurston remarked:—"Its power of doing work with a restricted supply of water, and with little loss of efficiency, is something unusual and something to be especially remarked as a valuable characteristic of the wheel"—in every respect bearing out what he had said regarding it. In the Table in the paper giving results of tests of wheels, it would be noticed that, in the column "Efficiency obtained," the results were even more favourable than those he had brought before the Institution. Of course, the Professor's tests were made some months later than his, and in the interim there may have been some improvement made upon the wheel. Mr Murray, in his paper, had taken exception to some of his statements, and took the opportunity to have a fling at the American turbine, especially on the score of cheapness. Mr Murray was a user of turbines—an American turbine, and possibly one which had been used very frequently with advantage—the Leffel by name; but the Leffel, under the conditions above named, shortness of water, was the lowest in efficiency but one which had come under his notice. That one was the Victor turbine, which

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had to be taken out and replaced by a Hercules wheel, which gave better results working at half-gate. This Victor turbine was put into the flume mentioned by Professor Thurston and tested, with the result that at full gate it gave 88 per cent. of efficiency; at three-quarter gate, 67; at five-eighths, 58; and at half-gate, 44 per cent.; whereas the Hercules wheel at half-gate gave 79 per cent. of efficiency, showing that with that opening it was 75 per cent. more powerful than the Victory with the same amount of water. It afforded him very great pleasure to find that he had been so well supported in the statements he had brought before the Institution in his paper last session by a gentleman whom he had never seen, and knew not.

Mr RICHARD MURRAY said he had hoped that Professor Thurston would have given them something to do away with the shaking of their heads, alluded to by Mr Turnbull; but unfortunately, in the paper he had communicated to the Institution, he had given them nothing they could well found upon, to improve their knowledge in the matter. Although he told them about the marvellous conception of the design of the Hercules wheel, yet in the beginning of his paper he said:—"Some questions are asked relative to the construction and principles of operation of the wheel, which the writer may be able to answer; and some doubt is intimated as to the accuracy of the apparatus and the method of test, which I am in a position to meet." For all that, he did not tell them where the power came rom that was said to be given out by the wheel; and he thought there was some slip or some flaw in regard to the useful effect, or indicated horse-power given out by the wheel. He gave them interesting, but not very instructive information. He noticed one peculiar item in the Table given by Professor Thurston. In the test of October 10th, 1883, and that of the 13th of the same month, which he understood referred to the same wheels, it would be observed that, taking these wheels at full gate, the horse-power given out at the 10th October was 147, whereas that on 13th October was 145, but the per centage of efficiency was lower in the former case than in the latter, although the horse-power was greater. Now, it appeared that there was some slip or error here,

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and he could come to no satisfactory conclusion from these tests in reference to the useful effects of American wheels. He did not assail the Hercules wheel. He merely spoke of the American turbines generally. He could not say that his expectations of the useful effect of turbines had not been fulfilled. He had lately started a turbine at Lanark, which was intended to give 400 horse-power, and he believed that it had really given from 380 to 390 horse-power. If it was proved to yield that useful effect he would not be disappointed with it; but that a Hercules wheel should yield 79 or 80 per cent. of useful effect he could not and would not accept. In his own experience he had no such instance. The Professor had given them little to increase their faith in his tests, as it appeared that he had accepted the opinions of others, at least he judged so, but he might be wrong in his judgment.

Mr TURNBULL said he thought Mr Richard Murray was under a misapprehension with regard to the figures given in Professor Thurston's paper. These were results got by Professor Thurston himself, and, therefore, no one had a right to doubt them. There was a test made of every "Hercules" wheel before it was sent out, and there was no wheel yet in this or any other country that gave such results. In answer to Mr Gale, Mr Turnbull further said that the testing apparatus was a wheel of about four feet in diameter, with a friction band over it. There was a diagram and full particulars of it in his paper of last session.

MR RICHARD MURRAY said he did not advocate any particular wheel. He merely drew attention to some he had fitted up. No particular wheel could overtake or meet the requirements of the different cases to which turbines had been applied. The right thing was to take the turbine best suited to the circumstances of the case. As to the reliability of tests, he had drawn attention to the fact that from the same flume, turbines tested by the same engineer and under the same conditions, varied from 17 to 18 per cent. in useful effect. With such variations before them he thought they might reasonably doubt the accuracy of the tests adduced by Professor Thurston.

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Mr JAMES M. GALE said that if the same dynamometer were used the results given would probably be a close approximation to the comparative efficiency of the turbines.

The PRESIDENT said he could well understand that the results given by turbine wheels would vary; for even with the same indicator on steam engines they got differences of effect, so that he thought they ought not to scrutinize the figures in Professor Thurston's paper too closely. If there were such differences as 17 per cent. of effect in similar turbines, as mentioned by Mr R. Murray, they could afford to let those smaller discrepancies pass. Then with regard to the remark in the paper on the "mechanic of the true Yankee type," it was well known that very often good mechanics could make a mill although they could not draw it on paper. In his own experience one of the most celebrated shipbuilders could not tell from looking at a model whether it was identical with the lines on the drawing, and yet this celebrated man found fault with the lines, although they were a true index of the model that was lying before him. That showed that they ought to allow a little latitude and not judge a mechanic by the amount of his theoretical education, as very often they found practical men deficient in that, but very good and useful in invention. He thought that the paper communicated by Professor Thurston, though short, contained a good deal of information, and, therefore, he was well entitled to a hearty vote of thanks.

Mr W. RENNY WATSON said he fully agreed with the President in his statement, and with Mr Turnbull that a natural mechanic many a time hit upon and worked out that which a highly educated man in the way of theory never thought of. He had found it so in his own experience, especially with American mechanics. He thought there was a great deal in the paper, and that they should not in the least depreciate what Professor Thurston had sent them. He had found that Professor Thurston's name stood in the first rank in America, as a man to whom improvements of this kind were referred to; and, therefore, he had no doubt that the data given in the paper were as correct as the circumstances would allow. He added his testimony to the paper and joined heartily in the vote of thanks to the Professor.

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Professor JAMES THOMSON said he concurred very much with Mr Watson in the belief that a clever mechanic might, in many cases, devise a good thing, such as a man of more theoretical education, and with eyes obscured perhaps with mathematical dust, might never have seen. There was indeed too much of dust and cobwebs of unsuitably applied mathematics encumbering many of the theoretical treatments frequently offered for designs and performances of turbines; and he thought good might be done by brushing much of that way. Still he did not think that the subject of turbines, with its numerous and intricate hydrodynamic questions, was in any case to be well worked out "by pursuing simply the intuitions of the natural mechanic" and "without the slightest knowledge of the theory of hydraulics."

The SECRETARY said it was only proper for him to state that Professor Thurston's letter accompanying his communication gave it only the character of a kind of addendum to Mr Turnbull's paper of last session. There was no attempt at anything elaborate in it.

The vote of thanks was then cordially awarded Professor Thurston for his paper.

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On the Properties of Screw Piles for Iron Piers, &c., considered Theoretically and Experimentally.

By Mr William Murray, C.E.

Received 13th November, 1883; Read 22nd April, 1884.

In the following theoretical investigations on the bearing power of screw piles, which also apply to similar foundations in any sort of ground, the writer has assumed that the earth is dry and pulverised and that the weight per unit bulk and the co-efficient of friction are known; if, however, the earth were taken as found, and broken up into very small pieces, so that its natural slope in this state could be ascertained, the same investigations would apply; but on the other hand when it became again consolidated as at first, the pressures would probably be different.

Let W = any weight.
„ ϵ = weight of unit bulk of earth.
„ x = any depth.
„ p = pressure per unit area (vertically).
„ p' = side pressure per unit area.
„ ϕ = angle of repose or friction.
„ f = co-efficient of friction.
„ r = radius of pile shaft.
„ R = radius of screw blade.
„ c = fraction of weight supported by the screw.

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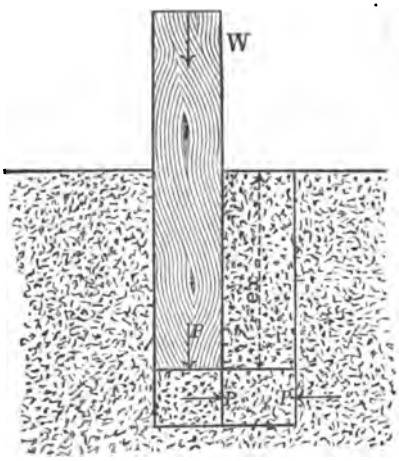

Fig. 1.

In considering a beam (Fig. 1), or any other material, a certain depth x, into dry pulverised earth, and acted on by a weight W, which produces a downward pressure p, and consequently a reaction equal and opposite, per unit area at the lower end, which in its turn produces a side pressure p' per unit area. In this case the following relations exist:—

$$\frac{p}{p'} \text{ lies between } \frac{1-\sin\phi}{1+\sin\phi} \text{ and } \frac{1+\sin\phi}{1-\sin\phi} \quad \ldots\ldots\ldots\ldots(1)$$

$$\frac{p'}{ex} \text{ lies between } \frac{1-\sin\phi}{1+\sin\phi} \text{ and } \frac{1+\sin\phi}{1-\sin\phi} \quad \ldots\ldots\ldots\ldots(2)$$

When $\frac{p}{p'} = \frac{1-\sin\phi}{1+\sin\phi}$, the beam is on the point of being forced upwards by the side pressure p', and when $\frac{p}{p'} = \frac{1+\sin\phi}{1-\sin\phi}$, the pressure p has been increased, so that the beam is just on the point of sinking. Similarly, $\frac{p'}{ex} = \frac{1-\sin\phi}{1+\sin\phi}$, when the beam is on the point of being forced upwards, and $\frac{p'}{ex} = \frac{1+\sin\phi}{1-\sin\phi}$ when the beam is on the point of sinking.

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Multiply (1) and (2), then

$$\frac{p}{ex} \text{ lies between } \left(\frac{1-\sin\phi}{1+\sin\phi}\right)^2 \text{ and } \left(\frac{1+\sin\phi}{1-\sin\phi}\right)^2 \quad \ldots\ldots\ldots\ldots(3)$$

That is, $\frac{p}{ex}$ can neither be less than $\left(\frac{1-\sin\phi}{1+\sin\phi}\right)^2$, nor greater than $\left(\frac{1+\sin\phi}{1-\sin\phi}\right)^2$.

From (2) the side pressure per unit area on the shaft of the pile at any depth $= ex\,\frac{1+\sin\phi}{1-\sin\phi}$. The friction on this area $= ex.f.\frac{1+\sin\phi}{1-\sin\phi}$, and the friction on a ring of the pile whose breadth is $dx = ex.f.\ 2\pi r.\frac{1+\sin\phi}{1-\sin\phi}.dx$. The friction or resistance to sinking on the whole shaft of the pile will therefore be equal to $\int ex.f.\ 2\pi r\,\frac{1+\sin\phi}{1-\sin\phi}.dx$. That is,

$$\begin{aligned} W - Wc &= ef\ 2\pi r\,\frac{1+\sin\phi}{1-\sin\phi}\int x.dx \\ &= ef\ 2\pi r\,\frac{x^2}{2}\,\frac{1+\sin\phi}{1-\sin\phi} \\ &= ef\,\pi r\,x^2\,\frac{1+\sin\phi}{1-\sin\phi} \quad \ldots\ldots\ldots\ldots(4) \end{aligned}$$

As in (3) $\frac{Wc}{\pi(R^2-r^2)\,ex} = \left(\frac{1+\sin\phi}{1-\sin\phi}\right)^2$

$$\therefore Wc = \pi(R^2-r^2)\,ex\left(\frac{1+\sin\phi}{1-\sin\phi}\right)^2 \quad \ldots\ldots\ldots\ldots(5)$$

Adding (4) and (5),

$$\begin{aligned} W &= ef\,\pi r\,x^2\,\frac{1+\sin\phi}{1-\sin\phi} + \pi(R^2-r^2)\,ex\left(\frac{1+\sin\phi}{1-\sin\phi}\right)^2 \\ &= ex\,\pi\,\frac{1+\sin\phi}{1-\sin\phi}\left\{fx + (R^2-r^2)\,\frac{1+\sin\phi}{1-\sin\phi}\right\} \end{aligned}$$

or equal to a weight which would just be upon the point of causing the pile to sink.

When a plate is introduced at a flange, and the pile filled with

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sand or other material, as in the sketch (Fig. 2) an additional resistance to sinking is obtained.

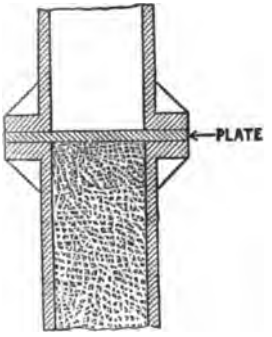

Fig. 2.

Let l = the length of pile filled with sand; also, let W′ and p'' = respectively the whole pressure and pressure per unit area on the under side of the plate. The latter pressure is less than the pressure per unit area at the bottom of the pile, by the weight of a column of earth of the same length and section.

$$p'' \pi r^2 = p \pi r^2 - le \pi r^2$$

$$W' = (p - le) \pi r^2$$

$$W' = \left\{ \left(\frac{1 + \sin \phi}{1 - \sin \phi} \right)^2 ex - le \right\} \pi r^2$$

$$= \left\{ \left(\frac{1 + \sin \phi}{1 - \sin \phi} \right)^2 x - l \right\} e \pi r^2.$$

The following experiments as to bearing power of ground for screw piling and practical results obtained during the construction of an iron quay with which the water was connected may be interesting. The experiments consisted in putting down a series of piles to such a depth as enabled them to support a weight equal to four times the working load, which averages 16 pounds per square inch of the area of the pile and screw over the pier.

While putting down the piles for the quay, it was observed that they did not attain anything like their full bearing power until

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after standing untouched 30 or 40 hours. In some cases where they had been sunk 25 or 30 feet, and loaded immediately after, they began gradually to sink with a weight of 8 to 9 tons; whereas, in similar cases, when allowed to stand 30 hours, they bore 30 tons without the slightest indication of sinking. From this it would appear that while the pile is being sunk, the flanges in its shaft, and more particularly the screw in their passage downwards disturb the ground so much that the pile cannot be said to have attained its ultimate bearing power until a sufficient time has elapsed for the earth to close up and exert its full side pressure around the shaft. This is also borne out in the screwing of the permanent piles, for as long as they were being turned, a force equal to 15 men at the end of a 12 feet lever was generally sufficient to screw them to their bearing. If, however, a pile had not reached its destination before night came on, and was allowed to remain till morning, it was with the greatest difficulty that it could be again started even by applying three times the usual power. On one occasion it was all that 36 men could do, jerking as hard as they could, to move a pile that had been standing for some time. This was very soon understood by the contractor, and, latterly, whenever the screwing of a pile was begun it was put down to its bearing without stopping.

In cases resembling that last mentioned, and where small beds of shells or stiff ground were encountered, the resistance offered was so great that the studs, which made the capstan bite the piles, cut deep grooves in them. This, however, was when the men at the end of the capstan bars threw their whole power into a series of jerks.

The time required for one turn of the screw varied from 3 to 20 minutes according to the nature of the beds to be perforated. The average time, when there was no peculiarity in the ground, was 8 minutes.

The uncertain nature of the ground and the irregularity of its composition rendered it a work of the greatest difficulty to keep the piles in their proper positions during the progress of screwing. Sometimes a portion was met with hard on one side and soft on the other, and in two or three turns of the screw the pile would be

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thrown out as much as 4 feet. This was the principal difficulty experienced; but the ground required to be watched carefully, for these hard pieces could sometimes scarcely be felt in boring with an iron rod.

By far the greater number of the piles rest on a few inches in the hard stratum already mentioned. Some, however, where the hard layer had disappeared, stood in the clay supported by friction on the pile in addition to what resistance is due to the screw blade. In such cases they were usually weighted with a test load, and when there was any doubt, they were sunk an additional length.

The length of the piles above low water flange, except for the first 20 spans, was constant throughout. The length in the ground was exceedingly variable, being in some cases only 13 feet, and in others as much as 50 feet, according to the depth of the hard stratum from the surface. The piles under low water flange were made up of 9 feet lengths, having in addition shorter pieces attached where required. A supply of 3 and 6 feet pieces were always kept ready to put on when longer pieces would have sent the screw through the hard bed into the soft clay below.

The discussion of this paper took place on the 22nd April, 1884.

Mr ARCHIBALD BARR said that he did not wish to enter at any length into the discussion of this paper, but merely to make one or two observations, bearing especially upon the theoretical part of the paper. He would remark in the first place that the mathematical investigation with which this paper opened was—if he might be permitted to parody some of the words just used by Professor Thomson—founded upon the properties of "mathematical dust," not upon those of the real earth to be dealt with in Engineering work. Mr G. H. Darwin, in a paper published in the Proceedings of the Institution of Civil Engineers (vol. lxxi., Session 1882-83, Part I.) has shown that, even in the case of dry sand carefully arranged for experiment, the horizontally exerted pressures at different depths are very uncertain, and they are much more so with ordinary earth,

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especially in such a case as that treated of in the paper, in which the earth whose pressures are to be estimated, has been disturbed by the sinking of the pile. In this connection he might mention that Mr Simms in his book on Tunnelling, stated that the pressures met with in his excavations were sometimes much greater in shallow ground than at greater depths. Again, before any results can be deduced from the equations it is necessary to know the angle of repose for the particular quality of earth through which the pile has to pass, in the particular condition in which it exists *in situ*. Now, the angle of repose is very variable, even for the same mass of earth under different conditions, as to moisture (with or without water pressure), and other disturbing agencies. For example, Mr Simms in his work on Levelling, stated that the slopes of a particular cutting through the London clay, which were originally two to one, slipped down from time to time until they were flatter than six to one. Mr Barr considered the latter part of the paper much the more valuable, and thought that in it there was sufficient evidence to show the untrustworthy character of all calculations founded upon theoretical considerations in this subject. It was essentially a subject which did not admit of reliable theoretical treatment.

Mr T. A. ARROL said that he quite granted that the mathematical formula given in the paper might be correct, but he would have liked if Mr Murray had given them also some practical results of tests, comparing them with the theory, which would have been of great use to them all. But he had no doubt that it would have taken him a much shorter time to put down the piles than to find the coefficient of friction. It would be very interesting to have placed on record the size of the piles and the pitch of the screw on the piles, and working out the comparison for them; because without that information the whole paper to his mind was useless. There must have been considerable extra friction caused in sinking the piles on account of the flanges, and they must have added greatly to the difficulty in starting the soil around the pile. Mr Murray seemed to state that when a plate was introduced between the flanges and sand allowed to fill the pile, the resistance to sinking was consider-

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ably increased. So far as he was aware, a plate fitted at the bottom of the pile would virtually have the same effect.

Mr W. RENNY WATSON said had Mr Murray been able to turn the pile round in a minute rather than in eight minutes it would not have required a force of 15 men to turn it, and he believed it would have gone down very much faster, on the same principle as Nasmyth carried out, his idea being that the pile should not be allowed to stop at all. The result was that Nasmyth got the pile down in a quarter of an hour, whereas previously, it had taken a man three-quarters of a day to accomplish the same work. Recently he had been in France and had seen them driving at 26 blows a minute, which kept the pile constantly moving, thereby indicating that it was much easier driving piles while a constant motion was maintained.

Mr T. A. ARROL said that the co-efficient of friction was a varying quantity, depending upon the nature of the soil through which the piles had to be driven.

Mr T. D. WEIR confirmed what Mr Watson had said. In sinking piles by means of the water jet as long as the pile was kept moving considerable progress was made; but whenever a stoppage ensued, great difficulty was experienced in starting the pile afresh.

The PRESIDENT said that if they took into account the number of strokes applied and the weight used, and knew the distance driven, a proportion between these various elements could be arrived at. No doubt if a body were kept moving it would be easier driven.

Mr W. RENNY WATSON said it was a singular fact that in the case he had already mentioned a much lighter weight than ordinary was used, with the result of a lessening of injury to the head of the pile. The pile driver was merely a steam-hammer, as ordinarily constructed by Nasmyth, resting on the top of the pile, and descending with it as long as its blows were given to the head of the pile, which thus was made to penetrate the ground in a surprising manner.

Mr T. D. WEIR said that at present on the Putney Bridge being put across the Thames the sheet piling was driven in the manner described by Mr Watson as being in use in France.

The discussion was then continued till next Session.

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On Fog Signals as applied to Lighthouses.

By Mr GEORGE H. SLIGHT, JR.

(SEE PLATE X.)

Received 30th October, 1883; Read 22nd April, 1884.

WITH the present Lighthouse system of this country the mariner is enabled to determine his position on approaching our coasts with as much certainty by night as he can by landmarks and beacons during the day. Without it, it would be absolutely impossible to carry on the enormous business which depends so much on our shipping.

But when these landmarks, beacons, and the strongest artificial lights are enveloped and completely blotted out by fog, other means must be used to acquaint the sailor with his position, and sound signals conveyed through the atmosphere as a medium, present the easiest method of communicating with him. Although the system of sound signalling is surrounded by so many difficulties that there is danger to the sailor if he attempt to trust entirely to its guidance.

It is therefore a melancholy fact that in spite of all the precautionary measures in use, the record of wrecks is still alarmingly great, and as a large percentage of these disasters is due to fog, it may justly be called "the seaman's worst enemy."

In these days of ever increasing trade, keen competition, and quick passages, there is great temptation for captains of vessels to keep up speed even during fog. This in a great measure increases the danger of navigation, and the Lighthouse authorities, although by no means encouraging recklessness, do their utmost to assist the mariner when

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the lights and landmarks are obscured. Fog, snow, and rain were, until recently, generally considered to offer serious obstruction to sound, while wind has always been justly regarded as having great power in intercepting and even diverting it.

The development of fog-signalling led to a more exact inquiry, with regard to the transmission of sound, and in the years 1873 and 1874, at the instance of the Trinity House authorities, a careful investigation was carried out, to gain more accurate knowledge of the effect of weather on the transmission of sound, and also to test the value of various sound producing instruments. This investigation was conducted by Professor Tyndall, the scientific adviser to the Trinity House, aided by a committee of the Elder Brethren and several of the officials of that Corporation.

The experiments were carried out very carefully, and extended over a considerable length of time; observations being taken in all states of the weather, and repeated again and again, and the information thus gained is of great interest and importance. The results are given minutely in Dr Tyndall's book on "Sound," but may be briefly summed up by saying that neither rain, hail, snow, nor fog has any appreciable power to obstruct sound. Wind, and what Dr Tyndall calls acoustic clouds, are the chief obstructors of sound.

Acoustic clouds are not visible clouds, but arise from currents of air differently heated or differently saturated with moisture, and often exist when the atmosphere is exceptionally clear; these clouds intercept and break up sound by repeated reflections, so it follows that a bright clear day is not necessarily the best for hearing distant sounds, and on a day of dense fog, it is quite possible that no obstruction is offered to the passage of sound, while it is more than probable that the cause of the uncertainty in determining the direction of a sound, arises from its reflection by those acoustic clouds.

A very elaborate series of experiments was also made by the Lighthouse authorities of the United States, when the late Professor Henry was chairman of the Lighthouse Board, and the results of the Trinity House experiments at South Foreland were mostly corroborated, with the exception of Professor Tyndall's theory of

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acoustic clouds, Professor Henry being of the opinion that the sound was affected by the presence of upper and lower currents of air, and the reflection of the sound from the surface of the sea and of waves.

In a paper read by Mr David Allan Stevenson, of Edinburgh, in 1881, he also seems to be of Professor Henry's opinion, but this being a distinctly scientific subject, I think it may with safety be left alone for this evening, it being sufficient to know that, from whatever cause, sounds are liable to be weakened or diverted in certain states of the atmosphere.

Bells, gongs, guns, rockets, whistles, reed trumpets, and sirens are all in use at the present time at the various fog signal stations round the coast, but at all important points where necessary and practicable, "sirens," which were found by Dr Tyndall to produce the loudest continuous sound, are superseding the less powerful instruments.

Perhaps the earliest recorded application of bells being used as warning signals is that mentioned in the traditionary story of the "Abbot of Aberbrothock," who was said to have moored a raft on which a bell was fixed, off the "Inch Cape Rock," afterwards called the "Bell Rock," and was rung by the motion of the waves. The "Bell Rock" lighthouse was erected on this dangerous reef in the year 1811 by Mr R. Stevenson, and two bells were fixed on the balcony. Since that date bells have been used at most rock lighthouse stations. For such places where the room is so limited, "bells" are a very suitable form of fog signal. The bells in use vary in weight from 3 to 45 cwt., and they are generally struck by means of clockwork, the clappers only being actuated by machinery; in no case does the bell itself move. The intensity of the sound depends on the size of the bell, and the weight and fall of the clapper, but the sound produced is not very powerful, although close at hand, it may seem to be so, and the effective range of the largest bells fluctuates very much; with the wind they may be heard as far as 10 miles, but against the wind they may be quite inaudible at less than a quarter of a mile.

Gongs have been chiefly used as fog signals on board light vessels. Those in use are of Chinese make, about two feet in diameter, and

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are struck with a stick having a padded head, the strokes given are short and in quick succession, causing a continuous roll; the sound is distinctive but only serviceable at short distances. As a rule, vessels may approach nearer to light vessels and rock stations, than to a rocky coast marked by a lighthouse, so that bells and gongs, though feeble, may be of great service; gongs however, are being superseded by more powerful instruments on board particular light vessels, while on others they will no doubt continue to be used for some time to come.

Guns have been largely used, as coast fog signals since 1856. Formerly the guns were fired during foggy weather every fifteen minutes, but recently the interval has been altered to ten minutes.

The labour of firing during a long spell of fog is very great, and to obviate this in some measure, experiments were made at Woolwich Arsenal in 1874-76, and Colonel Maitland of the Gun Factories, Woolwich Arsenal, designed a gun, breech-loading, with six chambers, somewhat similar to a revolver, and fitted with a parabolic mouth-piece to assist in directing the sound. This gun broke down shortly after being used at the North Stack near Holyhead, although it gave every promise of success, but no further attempt was made to repair it or make another, as the attention of the Trinity House was at this time directed to gun cotton as a sound producer.

Mr J. R. Wigham of Dublin, has also invented a very ingenious gas gun, and where a supply of gas is available, the apparatus is easily applied.

The gun consists simply of an iron tube about 18 inches bore and 12 feet long, placed at the point where the signal is required, and connected to the gas main or gas holder by iron piping. The gun is loaded with an explosive mixture of gas and atmospheric air, by simply turning on a cock; it is fired by applying a light to the shore end of the tube, and the explosion takes place almost immediately at the mouth of the gun.

Mr Wigham states that a gun may be fixed so far as half a mile from the loading and firing stations.

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Dr Tyndall gives his opinion of the ordinary gun as a fog signal thus—

"The duration of the sound is so short that unless the observer is prepared beforehand, the sound, through lack of attention, rather than through its own powerlessness, is liable to be unheard. Its liability to be quenched by a local sound is so great that it is sometimes obliterated by a puff of wind taking possession of the ears at the time of its arrival. Its liability to be quenched by an opposing wind, so as to be practically useless at a very short distance to the windward is very remarkable;" and further, "Still, notwithstanding these drawbacks, I think the gun is entitled to rank as a first-class fog signal."

Rockets are used at several Lighthouse Stations in England: they were introduced at the suggestion of Admiral Sir Richard Collinson, the Deputy Master of the Trinity House. The arrangement consists of three parts, the rocket, the detonator, and the gun cotton or tonite cartridge, which causes the chief explosion; the rocket carries the detonator and the cartridge up to the height of 600 feet or so, and then ignites the detonator which is placed in the centre of the cartridge, the explosion of which causes the report. The three parts are kept separate for safety until required for use, but can be fitted together in about two minutes. The advantage of this form of signal is that it can be made to explode above any obstruction which would cause a sound shadow, while the chief explosion takes place at a distance from the building. A variety of this form of sound signal consists merely of a gun cotton charge, and the detonator fired by electricity. It is adapted for Rock Lighthouse Stations where space and accommodation are valuable; experiments are still being carried on by the Trinity House to make this signal more complete. It has already been put into use at Flamborough Head, Lundy Island, the Smalls Rock in St. George's Channel, at Heligoland, and at the Tuskar Rock on the south-east coast of Ireland.

Whistles are not much used in this country for coast fog signals, although they are greatly used in the United States and Canada;

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with the exception of the Cloch Lighthouse in the Clyde, where Messrs Stevenson erected two small whistles in 1874, giving two different notes, sounded one immediately after the other, there are no fog signal whistles on our coasts. In the United States they have been in use at many places since 1851. The Lighthouse authorities in the United States took up the subject practically before it engaged much attention in this country, owing to the prevalence of fog on the east coast of America, and the hindrance caused thereby to the coasting traffic; and they first brought into use the huge whistles, trumpets, and "sirens."

During the Trinity House experiments in 1873-74 it was proved that the sound of the most powerful whistles, blown either by steam or compressed air, was generally inferior to the sound of the other instruments—guns, reed trumpet, and siren. The largest whistle used was one 12 inches diameter and 20 inches high, and the pressure of steam was 74lbs.

The whistles used in the United States and Canada, vary in size from 6 inches to 10 inches in diameter, and about 18 inches high, sounded with pressure from 65lbs. to 80lbs.; the apparatus used being a steam boiler and a small engine for opening and shutting the valve so as to admit and cut off steam periodically to the whistle. The sounding of the whistle is caused by the vibration of the column of air contained within the dome or bell of the whistle by the impact of the steam, or in some cases by a current of air at a high pressure, and probably the metal is also set into vibration. Difference of pitch is got by altering the distance between the steam orifice and the rim of the dome, when brought close together the sound is very shrill, but it becomes deeper as the space is increased between the steam or air orifice and the rim of the dome.

Many remarkable reports have been circulated as to the great distance the American and Canadian whistles, or *roarers*, have been heard: 30 and 40 miles was said to be no unusual distance with the wind, and even 20 miles against it. Perhaps the American atmosphere is more favourable to the transmission of sound, but in this country the best American and Canadian whistles have never pro-

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duced sounds capable of being heard to such distances, and indeed at the South Foreland experiments, the whistles were generally behind the gun, trumpet, and siren.

The want of penetrative power in the whistle is probably due to the sound being diffused equally all round, and to some extent wasted in the immediate vicinity of its production.

Daboll's fog horn or reed trumpet was introduced into this country in 1862, when the Trinity House gave it a practical trial at Dungeness, and it was found to give very satisfactory results. It consists of a brass trumpet from about 6 to 10 feet in length, with a large trumpet mouth, and having at the small end a box or chamber in which a strong steel tongue, or reed, vibrates, similar to the reed of a clarionet. This tongue varies according to the size of the trumpet, and is from 2 inches to 4 inches wide, 9 inches to 18 inches long, by $\frac{1}{4}$ inch to $\frac{3}{8}$ inch thick. It is held fast by two screws at one end, and is tuned to the fundamental note of the trumpet by means of a sliding block, which rests on the reed and lengthens or shortens the vibrating portion as required. The usual way in which the horn is caused to sound, at regular intervals, is to have it mounted on an air vessel of a certain capacity, so that in a given time, an air pump may raise the pressure say to 10 lbs., and immediately on its reaching that point, a loaded valve or piston rises and opens the passage to the horn, which is blown until the pressure falls say to 6 lbs., when the valve is closed automatically, and so on.

Daboll used a caloric engine on Ericcsen's principle for working the air-pump, and if the engine works at a regular speed, which it does as a rule, this arrangement keeps time with wonderful accuracy.

Professor F. H. Holmes considerably improved this instrument, especially with regard to the automatic arrangement, whereby the signal is sounded.

Various devices have been proposed for working fog horns, both by steam and hand power, and, although they have been largely used in America, they are practically done away with in this country, and require no more than a passing notice.

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A very simple hand fog horn is used in some cases on board light vessels, &c. It is made by the Holmes' Marine Life Protection Association, and consists of two cylinders of tin plate, one sliding over the other, the outer finished with a bent trumpet mouth, the inner carrying a reed mouthpiece and portion of a trumpet, and is made to rest on the deck, where it is held by the feet of the operator, who raises the outer portion by hand, thereby filling the inner chamber with air, which is blown through the reed when the trumpet is forced down, when a tolerably loud sound is produced. The pumping motion is repeated at intervals as required.

We now come to the most important of the instruments used as fog signals, *i.e.*, the "Siren."

It was originally invented by Cagniard de la Tour, and has long been known and used on a small scale in connection with experiments on sound, but until Messrs A. & G. Brown of New York took the matter in hand, it had never been tried of such a large size.

Messrs Brown introduced the instrument in 1871, and one of their sirens was sent to this country and tested with the other instruments at the South Foreland experiments.

The new siren was comparatively small, consisting of two discs about five inches in diameter, each with 12 radial slots, one disc fixed and the other mounted on a horizontal spindle, so that it could be made to revolve by means of a pulley and band in connection with a convenient motive power. The siren was fitted to the small end of a nearly conical, horizontal, trumpet of cast iron about 16 feet long, and about 30 inches in diameter at the large end, and when required to be sounded, steam from a H.P. boiler was blown through it.

The interruption to the blast caused by the revolution of the disc, produced a rapid succession of puffs or pulsations of steam into the trumpet and so produced a very loud and powerful sound, the vibrations not being taken up by the cast iron trumpet, the sound issues from the mouth in a condensed beam of great intensity. The apparatus, as received from Messrs Brown, included a small steam engine for driving the siren and for feeding the boiler

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with water; the siren ran continuously, but only sounded when steam was passed through it. It was considered that the best sound was produced when the siren made 2,400 revolutions per minute, this makes 2,400 × 12 = 28,800 pulsations per minute, or 480 per second.

Dr Tyndall describes this instrument as being "beyond question the most powerful fog signal which has hitherto been tried in England." It was considered to be so superior to all the others, that it was ordered to be adopted generally for the Trinity House Service, with the difference that instead of steam, compressed air was to be used, and for this purpose the Trinity House decided to use caloric or hot air engines, similar to those used by Daboll for his fog horn. These engines, although of small power and more liable to get out of order than steam engines, are entirely free from risk of explosion. A great advantage connected with them is, that their management may be very easily learned, as any intelligent seaman, after a few days instruction, is quite competent to work them.

The siren, since its introduction from America, has undergone several modifications and improvements: the first change was one in which the siren, instead of two discs, was composed of two cylinders, one fixed and the other revolving. By this arrangement, without increasing the diameter of the instrument, a larger surface was made available for the slots than could be got with a disc, and thus the number of slots could be increased from 12 to 30 or 36, with the advantage of not only increasing the cover or relative space between the slots, so as to make a better separation of the puffs or pulsations, but also to diminish the speed at which the siren was driven. Thus we have seen that to produce the standard number of 480 puffs per second, the disc siren required to be driven at 2,400 revolutions per minute, whereas a cylindrical siren with 36 slots only requires to make 800 revolutions per minute to give the same note, thus reducing the power required to drive it, and lessening the wear and tear. A number of sirens of this form are in use, and have done good service. The next improvement was patented by Professor Holmes, in which the cylindrical siren was adopted, but, in

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place of driving it by hand, he applied the principle used in the old experimental sirens of bevelling the slots, so that the motion of the air through them caused the inside cylinder to revolve, and to prevent its running away at an ever-increasing speed, he applied a brake, with hinged vanes, revolving among the steam or compressed air, the retardation being increased by frictional brakes, in connection with the vanes, and rubbing on the interior of the case. One great advantage gained by this was its running only while being sounded, it being started by the admission of steam or air. If the slots of the fixed and revolving portions happened to coincide when the instrument was at rest, the mere admission of the compressed air was sufficient to set it in motion, but for fear of their not coinciding, an ingenious arrangement was supplied for imparting motion to the revolving cylinder, so as to insure the coincidence of the slots, and then allowing the air to drive the cylinder. A ratchet was formed on the spindle of the siren, and the entrance to the air-chamber round the siren was closed by a valve, kept in position by a light spiral spring, so that whenever the air forced the valve open, a light tongue attached to the valve caused the ratchet and the siren to make a partial revolution, when immediately the siren started at its proper speed, but prevented from running too fast by the action of the brakes. This instrument has done good service by saving the driving power required for previous forms of siren, but has been found troublesome in practice, from the wear of the ratchet and the breaking of the starting tongues. Another difficulty was sometimes felt if a little oil or grease found its way into the brake, as it then ceased to act properly, and the siren running away, a shrill squeaking note was produced.

Mr Slight, the Superintendent of the Trinity Workshops, Blackwall, who has had a great deal to do with the practical application of sirens in this country, has since introduced a much simpler starting arrangement, which has been found more reliable, in which the starting valve, ratchet, and tongue have been laid aside, and in their stead two of the divisions between the slots, one diametrically

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opposite the other, are formed with two or three extra slots, so that in whatever relative position the fixed and revolving portions may be while standing, there must always be at least one passage for air on each side, open right through, and when the admission valve is opened, these allow sufficient air to pass, to start the siren. A further improvement is the substitution of a centrifugal governor for Professor Holmes' brake, arranged so that there is no friction at the starting of the siren, but as soon as the proper speed is attained, the centrifugal force overcomes the elasticity of certain springs, and cork rubbers coming in contact with a fixed ring, produce sufficient friction to prevent the required speed being exceeded.

The most recent alteration and improvement of this form of siren, is in causing it to produce two distinct notes, one high and one low, by increasing its length, and forming in the two cylinders two different sets of slots, and a diaphragm on the outer cylinder, between the two sets of slots, forms two separate air-chambers; the air, by means of two admission valves, is admitted to one portion only of the siren at a time, and as one has a larger number of slots than the other, the speed of the siren being always the same, or nearly so, from the action of the governor, the sound produced is high or low, according to the number of the slots; the larger number giving a high note and the smaller number a low note. The opening of the valves can be accurately regulated by clockwork, and a considerable variety in the signals can be produced by groups or series of sounds at intervals of, say, one, two, or three minutes, such as high, low; low, high; high, low, high; high, high, low; low, high, low, &c.

It has been stated that the trumpet through which the siren sounded was at first a straight horizontal tapered tube, but as it was considered necessary to direct the trumpet against the wind there was a difficulty in connecting it with the air reservoir, &c., so that it might be readily turned round to the wind; and to overcome this, the siren has been placed vertically, and the trumpet made partly vertical and partly horizontal, with a

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round bend sometimes near the siren and sometimes near the mouth, as might be most convenient.

In the most recent fog signals, the siren is bolted up to the bottom of the trumpet, and to the side of the siren is bolted an admission valve, patented by Professor Holmes, in which a very small valve, that can be opened by the pressure of the finger, admits the compressed air to a balance valve, by which the air itself opens the large valve leading to the siren. When high and low notes are given, two such valves are used, and the small valves are connected with a piece of clockwork, which keeps correct time by means of a weight and pendulum, so that the valves are opened and closed with absolute regularity.

To provide against any accident to the siren, it can be easily detached from the trumpet and admission valve, and a spare one substituted, with scarcely any interruption to the signal.

In conclusion, I may add, that there are now about 40 sirens in use at various lighthouses and light vessels round the British coasts.

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On Approximation to Curves of Stability, from Data for Known Ships.

By Messrs F. P. PURVIS and B. KINDERMANN.

(SEE PLATES XI.—XVII.)

Held as Read at Annual Meeting of 22nd April, 1884.

THE papers at the recent session of the Institution of Naval Architects show how much the subject of stability is exercising the mind of all who are responsible for the designing of ships. The object of many of those papers is practically identical, though the methods suggested vary; that object being to give rapid means of obtaining the stability of any given ship for all conditions of draught, provided that the centre of gravity of the ship be known. All the means suggested involve actual measurements from the given ship, and must, under the most favourable circumstances, require a considerable expenditure of time.

The object of the present paper is to introduce a method of approximation, by means of which the data obtained for one ship may be made available for estimating the stability of another. The word introduce is used advisedly, the results at present given being obtained from one ship only. It will be necessary that these results should be supplemented by results obtained from other ships of extreme variations of form; and one object of this paper is to suggest to other investigators to undertake the labour thus involved with a view to the ultimate good to be derived therefrom.

An analogy may be drawn between this paper and one by Mr William Denny, before the Institution of Naval Architects, in 1882,

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"On the reduction of transverse and longitudinal metacentric curves to ratio curves." In that paper two series of ratio curves (one for transverse, the other for longitudinal metacentre) are given. In the present paper curves analogous to one such ratio curve are produced; *i.e.* to say, the curves accompanying apply strictly only to one *type* of ship, using the word type in a sense which will be best understood a little further on.

It seems best to arrange the matter of this paper under the three following heads:—

(1) Considerations by which data for one ship may be made available for others of the same type.
(2) Explanation of the curves appended.
(3) Method of using the curves, for approximating to the stability of a ship of given dimensions.

We will treat of these heads in the order named—(1) Considerations by which data for one ship may be made available for others of the same type. These considerations may be arranged under three sub-heads—

(a) Neglect of length, which in ships of the same type has no effect upon the arm.

(b) Reduction of the breadth of the type ship to a unit value, say 100 feet. A ship for which data is possessed, if brought to 100 feet in breadth, would have a definite depth; all the measurements upon the sections of such 100′ ship would bear the same ratio to those upon the sections of the unreduced ship as the ratio of the breadths; *e.g.* draughts, heights of centre of buoyancy and metacentre, and lengths of righting arm would all be greater or less in the ratio of 100 to the breadth of the original ship.

(c) Variation of the constant depth just referred to, keeping the breadth unit—100 feet—fixed. This method of varying

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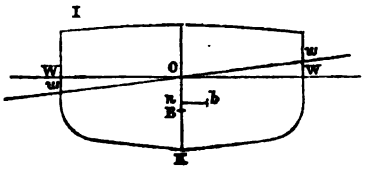
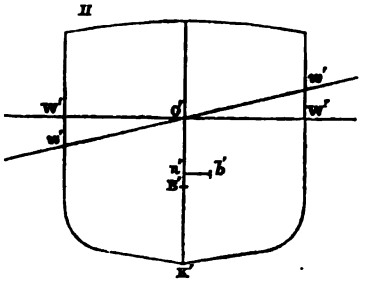
requires a little explanation. Suppose I. the section of the ship, for which calculations have been made, but altered in



all its proportions until the breadth has the unit value of 100 feet. Then if, keeping the breadth fixed, the section be increased—say doubled—in depth, as in II.



all the horizontal measurements remaining constant, and all the vertical measurements being doubled, the following relations will hold :—

The sectional area of II. will equal twice that of I.

Any water line in II. (W'W') placed at the same relative height as one in I. (WW) will give twice the area.

Any inclined water line in II. (*w'w'*) placed at the same relative height as one in I. (*ww*) will give twice the area, provided that tan. *w'Ow'* = 2 tan. *wow*.

With the same proviso, the ordinates of all centres of buoyancy of the section (B, *b*; B', *b'*) upright and inclined, in the depth direction will be doubled; the co-ordinates, in the breadth direction, will be unaltered: *e.g.*, *bn* = *b'n'*; B'K' = 2 BK; *n'*K' = 2 *nk*.

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The height of the metacentre above the centre of buoyancy will be halved; this is not so clear as the other matters just enumerated; but springs from the fact that, while the breadth of sections remains constant, the area (or divisor) is doubled.

It will easily be inferred that what is stated above as true for a section is true also for a ship; this is obviously the case, on the assumption that each section is varied in the same manner as the one treated, since the ship is simply an agglomeration of such sections. As used in this paper, the word "type" includes any ship derived from the original ship by variations of the nature above described.

(2) Explanation of the curves appended.

The curves give the righting arms at various angles for a ship of any length, of a definite breadth of 100 feet, of a depth bearing any ratio to the breadth, and having a draught bearing any ratio to the depth. They are obtained by help of the considerations enumerated above, and are applicable strictly only to ships of the same type.

They are set off on the assumption that the centre of gravity is in every case at the metacentre for the upright position.

To produce a curve of stability from them, it is necessary to take a reading for each of the angles: for this reading the ratio of depth to breadth will be needed, and the ratio of draught to depth, these two ratios determining the point at which the reading has to be taken, and the value of such reading. The readings, when set off at *abscissæ* representing the angles, give the curve of stability for centre of gravity co-incident with metacentre; a correction of the ordinary nature (viz., metacentric height × sine of angle) gives for each angle the amount to be added to the readings already obtained.

For the height of the metacentre for any ship of the type form the curves on Plate XVII. are given. The height of the centre of buoyancy is given by curve AA; for reading it the ratio of draught to depth is necessary, and at the abscissa denoting that ratio will be

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found for ordinate the ratio of height of centre of buoyancy to depth of ship. The height of metacentre above centre of buoyancy is given by curve BB; the abscissæ of this curve are also the ratios draught divided by depth; the ordinates require for their interpretation to be multiplied by the square of the breadth of the ship, and divided by the depth.

The ship from which the curves were constructed had a mean sheer of ·152 foot per foot of depth; and since all depth measurements conserve their ratio throughout the changes made, this ratio of sheer to depth is to be attributed to any ship the results for which are obtained from the curve.

Three things are hoped from the publication of these curves—(a) That they may form the nucleus of further work on the subject; (b) That they may serve as a check upon stability curves newly obtained for any ship by accurate methods; (c) That, although obtained only for one type of ship, they may, with caution, be employed for the purpose of approximating to the stability of ships differing not too much from the type form.

(3) Method of using the curves, for approximating the stability of a ship of given dimensions—

(a) Ignore the length of the ship.

(b) Find the ratio of draught to depth, and of depth to breadth; the latter in the form of a percentage, giving therefore the depth of a similar ship 100 feet broad. The depth* to be taken is from the top of keel to top of wood deck at the side of the ship, at the lowest part of the sheer, and the breadth is to exclude the thickness of shell plating.

(c) If there does not exist, for each angle, a curve already constructed for the correct draught to depth ratio, construct such curve from the companion set. Measure from the curve (existing or constructed) the righting arm at the correct ratio of depth to breadth. The result is the

* See footnote on page 195.

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value of the righting arm GZ for a ship 100 feet in breadth, and on the assumption that the centre of gravity is coincident with the metacentre for the upright position of the ship.

To obtain from this value the GZ of the real ship, two things more are necessary; to reduce the value obtained in the ratio of 100 to the real breadth of the ship, and increase it by the addition of GM × sin angle. The final result thus obtained for each angle gives the righting arm at that angle for the ship desired.

To show further the application of the curves, let us suppose that the curve of stability is required for a ship of dimensions 300′ × 40′ × 28′0″, the breadth and depth being measured as explained, viz.—breadth exclusive of shell plating, and depth top of keel to top of wood at side of deck; let us suppose that such ship is known to be not very different in form from the type form. Let the draught, excluding keel, for which the curve of stability is required, be 20 feet, and at that draught let us suppose that the height of metacentre is known to be 18·15 feet,* and height of centre of gravity 16·03 feet, so that GM = 2·12 feet. The ratio of depth to breadth is 70 per cent.; the ratio of draught to depth ·714. In this case, while there do not exist curves for the correct draught to depth ratio, there do exist curves for the correct depth to breadth ratio, *i.e.*, for a ship 70 feet deep, and it is only necessary to take readings where the abscissa draught divided by depth = ·714. The readings and subsequent work may be tabulated as follows:—

	Reading for Curves.		Righting Arms for real ship.	Add GM × sin angle = 2·12 × sin angle.	GZ.
15°	·08	× $\frac{40}{100}$ =	·03	·55	·58
30°	·82	,,	·33	1·06	1·39
45°	·80	,,	·32	1·50	1·82
60°	— ·52	,,	— ·21	1·84	1·63
75°	—2·50	,,	—1·00	2·05	1·05
90°	—4·80	,,	—1·92	2·12	·20

* The curves on Plate XVII. give for the type ship 18·31; viz.—·397 × 30·3 = 12 03 for height of centre of buoyancy, *plus* 1·19 × 40² ÷ 30·3 = 6·28 for height of metacentre above centre of buoyancy. The difference in this respect is not very great.

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In the second column are the readings obtained from the curves; in the fourth column these readings are reduced to figures applicable to the ship for which the approximation is made; and in the last column the righting arm is corrected for the true position of the centre of gravity of the ship.

The following co-efficients are derived from the actual ship forming the basis upon which the work of this paper has been founded. They are necessary, along with the sections given on Plate XI., for defining the features of any "type" ship. Similar co-efficients should be worked out for any ship for which approximation is desired, in order to see how far that ship resembles the type form.

Draught excluding keel, ÷ Depth.	Prismatic Co-efficient.	Midship Area Co-efficient.	Water-line Co-efficient.
·25	·563		
·50	·659	·912	·742
·75	·684		

Tumble-home on midship section ÷ breadth = ·019

Sheer* at fore perpendicular ÷ depth = ·218 } mean = ·152

„ „ after „ „ = ·086 }

Rise of floor „ = ·044

Round of beam „ = ·030

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*The sheer is an important element. If the sheer of the ship of which the stability is required is greater or less than that given above, a correction must be made upon the depth from top of keel to top of wood deck at side. To do this approximately, take excess or defect of mean sheer over or from ·152 × depth as described; take say ¼ of this excess or defect, and add to or deduct from depth; use the new figure so obtained instead of the depth previously described.

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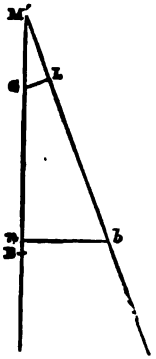
Appendix.

In the course of the many investigations made by Messrs William Denny & Bros., use has been made of measurement by co-ordinates to fix the position of centre of buoyancy for a given displacement and for any inclination of the ship. The length of the righting lever can readily be obtained from those co-ordinates; if, for



instance, Bn and bn be the co-ordinates, GZ is given by the equation.

$GZ = bn \cos \alpha + Bn \sin \alpha - BG \sin \alpha$; and in this equation, where B represents the centre of buoyancy when the ship is upright, all that is required is the height of centre of gravity of ship above the centre of buoyancy to make the righting arm easily determined.

For the construction of the curves, these co-ordinate measurements have, for the ship worked from, been treated as shown on the accompanying specimen, with the view to make them applicable to a ship 100 feet broad and of depth varying from 10 feet to 100 feet. The dimensions of the actual ship worked from were 42 feet broad and 30·3 feet deep, taking the depth from top of keel to top of wood deck at side of ship. In the case represented by the specimen given, the actual ship had a draught of 23 feet from top of keel; and the specimen shows the depth increased from 30·3 feet to 100 feet, and the draught consequently from 23 feet to 75·9 feet. In the column (1) are

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given the angles of inclination to which the co-ordinate measurements belong; in column (4) the new angles produced by consideration (*c*) in the body of the paper. In columns (5) and (9) are the horizontal and vertical co-ordinates of the centre of buoyancy of the actual ship, the one measured from the middle line, and the other from the centre of buoyancy in the upright position. Columns (6) and (10) give the new co-ordinates applicable to a ship of 100 feet beam and 100 feet depth; they are produced by the considerations (*b*) and (*c*) in the body of the paper, and must be referred to the new angles tabulated in column (4). In columns (8) and (12) these new co-ordinates are multiplied by the cosines and sines respectively of the new angles of inclination; in column (13) the sum of these products is made, and in (15) the deduction from this sum of amount due to the height of metacentre for the upright position above the centre of buoyancy; for the last named height the BM of the original ship (6·61) has been increased in the ratio of the breadth, and then decreased in the same ratio as the depth of 100 feet is greater than the depth of 72·14 feet, which would be the depth of the ship of 100 feet beam exactly similar to the original.

Ten sheets of the same nature as the specimen have been used for each condition of draught or displacement of the original ship; these ten giving new depths, varying from 10 feet to 100 feet, and new draughts always proportional to the depth. Eight such conditions of draught have been worked out, these draughts varying for the original ship when in the upright position between 8 feet and 27 feet.

For each of the eighty ships for which the stability data was thus obtained, it was necessary to make curves of stability, the abscissæ being those tabulated in column (4), and the ordinates those in column (15), on the sheets of which one specimen has just been described. These curves are not shown, but from them the measurements were made, by which the curves given have been constructed.

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Specimen.

Actual Ship modified from 42′ (broad) and 30′·3 (deep) to 100′ and 100′ (= d).
=100′ : 72′·14

Actual Draught from top of keel, 23′; of modified form $\frac{d = 100}{30{\cdot}3} \times 23'$. = 75′·9.

„ BM 6′·61

Angles.	Tans.	$\frac{d}{72{\cdot}14} = \frac{100}{72{\cdot}14}$	Products.	New Angles (α).	Horizontal Readings when B = 100.	Cos α.	Products.	Vertical Readings when d = 100.	Sin α	Products.	Sums of Products.	$BM \times \frac{100}{42} \times \frac{72{\cdot}14}{100} \sin\alpha - (11{\cdot}355) \sin\alpha$	MZ.
(1)	(2)		(3)	(4)	(5) (6)	(7)	(8)	(9) (10)	(11)	(12)	(13)	(14)	(15)
15°	·2679		·3718	20° 22′	$1{\cdot}77 \times \frac{100}{42} - 4{\cdot}21$	·9375	3·95	$·14 \times \frac{d=100}{30{\cdot}3} = ·46$	·3480	·16	4·11	3·95	·16
30°	·5774		·8003	38° 40′	3·51 8·36	·7808	6·53	·90 2·97	·6248	1·86	8·39	7·09	1·30
45°	1·0000		1·386	54° 12′	4·79 11·40	·5850	6·67	1·82 6·01	·8111	4·87	11·54	9 21	2·33
60°	1·7321		2·401	67° 23′	5·60 13·33	·3846	5·13	2·88 9·34	·9231	8·62	13·75	10·48	3·27
75°	3·7321		5·173	79° 4′	5·99 14·26	·1897	2·71	3·85 12·70	·9818	12·47	15·18	11·15	4·03
90°	∞				6·12 14·57			4·68 15·28		15·28	15·28	11·35	3·93

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Institution of Engineers and Shipbuilders

IN SCOTLAND

(INCORPORATED).

TWENTY-SEVENTH SESSION, 1883-84.

MINUTES OF PROCEEDINGS.

THE FIRST GENERAL MEETING of the TWENTY-SEVENTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 23rd October, 1883, at 8 P.M.

Mr JAMES REID, President, in the Chair.

The Minute of Annual General Meeting of 24th April, 1883, was read and approved, and signed by the President.

The Medals awarded at Annual General Meeting of 24th April, 1883, for Papers read Session 1881-82, were presented, viz.:—

The Institution Medal, to Mr JOSEPH MOORE, C.E., for his Paper on "Hydraulic Machinery for Deep Mining."

The Marine Engineering Medal, to Mr JAMES HAMILTON, Jun., for his Paper on "The Comparative Commercial Efficiency of some Steam Ships."

The Railway Engineering Medal, to Mr CHARLES PULLAR HOGG, C.E., for his Paper on "St. Enoch Railway Station."

The PRESIDENT delivered his Introductory Address.

On the motion of Mr ROBERT DUNCAN, Vice-President, a cordial vote of thanks was awarded the President for his address.

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The Discussion of Mr JOSEPH MOORE'S Paper on "The Use of Malleable Iron Water Pipes of Large Diameter in California," took place, and was continued to a future meeting.

A Paper on "The Stability of Ships at Launching," by Mr J. H. BILES, was read, the discussion of which was deferred till next General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows :—

AS MEMBERS :—

Mr WILLIAM LOCKHART BONE, Mechanical Engineer, Manchester.
Mr WALTER CHAMBERS, Consulting Engineer and Marine Surveyor, Belfast.
Mr HENRY DYER, Mechanical Engineer and Professor, Lenzie.
Mr GILBERT HENRY GARRETT, Mechanical Engineer, Glasgow.
Mr JAMES M'CREATH, Civil and Mining Engineer, Glasgow.
Mr ANDREW SPROUL, Mechanical Engineer, Greenock.

AS AN ASSOCIATE :—

Mr JOHN BARR, Secretary to Glenfield Co., Kilmarnock.

AS GRADUATES :—

Mr HARRY W. DOWNES, Draughtsman, 15 Rutland Cres., Glasgow.
Mr DUNCAN FINLAYSON, Apprentice Shipbuilder, 1 Osborne Place, Govan.

THE SECOND GENERAL MEETING of the TWENTY-SEVENTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 20th November, 1883, at 8 P.M.

Mr JAMES REID, President, in the Chair.

The Minute of General Meeting of 23rd October, 1883, was read and approved, and signed by the President.

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The PRESIDENT said that most of the gentlemen present would be aware that the Council had agreed, and were morally bound, to support the institution of a lectureship in anticipation of a Chair of Naval Architecture in the University. Some time ago the proposal was brought before the Institution by the Council and homologated. Mrs. Elder had now given a sum of money to endow a Chair of Naval Architecture in the University, but the Institution was still morally bound, and the Council had agreed, to continue the naval lectureship in connection with the Chair. The Council had resolved to raise a sum of money for this purpose. As the members were aware, Mr. Lawrie had been giving these lectures, and it now fell to the Institution to carry out their original intention of raising the money to endow the lectureship. In the meantime he desired to make this announcement, so that it should be known that the original intention was still to be carried out. Since Mrs. Elder had acted so handsomely in connection with the endowment of the Chair at the University, he thought the Institution could not do less than endow a lectureship. It was believed that a sum of about £5000, yielding about £200 a year, would secure a very able man to give the lectures. He thought that the Institution should acknowledge Mrs. Elder's generosity in this matter, and if that was agreed to, the Secretary would prepare a letter to be forwarded to that lady.

The suggestion was cordially agreed to.

The Discussion of Mr JOSEPH MOORE'S Paper on "The Use of Malleable-Iron Water Pipes of Large Diameter in California," was resumed, and was continued to a future meeting.

The Discussion of Mr J. H. BILES' Paper on "The Stability of Ships at Launching," was proceeded with, and was continued till next General Meeting.

Diplomas of Membership were presented to gentlemen elected at previous General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows:—

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AS MEMBERS:—

Mr JAMES DENHOLM, Mechanical Engineer, 360 Dumbarton Road.

Mr JOSEPH MOORE, Mechanical Engineer, San Francisco.

Mr W. L. C. PATERSON, Superintendent Engineer, 19 St. Vincent Crescent.

Mr GEORGE W. REID, Mechanical Engineer, Highland Railway, Inverness.

AS GRADUATES:—

Mr WILLIAM R. LESTER, Apprentice Engineer, 2 Doune Terrace.

Mr HUGH REID, Student Engineer, 10 Woodside Terrace.

THE THIRD GENERAL MEETING of the TWENTY-SEVENTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 18th December, 1883, at 8 P.M.

Mr JAMES REID, President, in the Chair.

The Minute of General Meeting of 20th November, 1883, was read and approved, and signed by the President.

The Discussion of Mr J. H. BILES' Paper on "The Stability of Ships at Launching," was proceeded with, and was continued till next General Meeting for the purpose of hearing Mr BILES' reply.

Mr LAURENCE HILL read a Paper descriptive of an "Improved Railway Chair Key and Fish Joint," and illustrated same by a full-sized model. A discussion took place and was continued to next General Meeting.

A Paper on "The Compound Engine Viewed in its Economical Aspect," by Mr R. L. WEIGHTON, was read, the discussion of which was deferred to next General Meeting.

Diplomas of Membership were presented to gentlemen elected at previous General Meeting.

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The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows :—

AS A LIFE MEMBER :—

Mr ALEXANDER EDWARD STEPHEN, Marine Engineer, 12 Park Terrace.

AS MEMBERS :—

Mr J. CAMERON ARROL, Mechanical Engineer, 18 Blythswood Sq.

Mr SAMUEL CRAWFORD, Shipbuilder, Clydebank, near Glasgow.

Mr LAWSON FORSYTH, Mechanical Engineer, 10 Grafton Square.

Mr THOMAS NICOL, Shipbuilder, Clydebank.

Mr GEORGE THOMSON, Ship Draughtsman, 9 Buckingham Terrace, Partick.

AS AN ASSOCIATE :—

Mr WILLIAM M'IVOR MORISON, Rocklee, Marine Place, Rothesay.

AS GRADUATES :—

Mr SEYMOUR HAMPDEN BEALE, Ship Draughtsman, 19 Hayburn Crescent, Partick.

Mr LUDWIG BENJAMIN, Draughtsman, Westburn Place, Whiteinch.

Mr DAVID BLAIR, Ship Draughtsman, 28 Rosevale Street, Partick.

Mr EBENEZER HALL BROWN, Draughtsman, 953 Govan Road.

Mr WILLIAM DENHOLM, Ship Draughtsman, 17 Rosevale Street, Partick.

Mr JOHN JAMES FERGUSON, Apprentice Engineer, 8 Walworth Terrace.

Mr PETER M'COLL, Ship Draughtsman, 3 Stewartville St., Partick.

Mr JOHN M'DONALD, Engineering Draughtsman, 293 New City Road.

MR JOHN BOW M'GREGOR, Ship Draughtsman, 22 Church Street, Partick.

Mr CHARLES W. MILNE, Draughtsman, 7 Carmichael Street, Govan.

Mr JAMES LANGMUIR NAPIER, Mechanical Engineer, 22 Salisbury Place, Hillhead.

Mr GEORGE SIMPSON, Ship Draughtsman, 13 Maxwell St., Partick.

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Mr LEWIS TAYLOR, Draughtsman, 2 Salisbury Terrace, Hillhead
Mr NICOL THOMSON, Ship Draughtsman, 39 Kelvinhaugh Street.
Mr JOHN WHITEHEAD, Engineering Draughtsman, 9 Glasgow Street, Hillhead.
Mr DAVID WOOD, Ship Draughtsman, 124 West Nile Street.

THE FOURTH GENERAL MEETING of the TWENTY-SEVENTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 22nd January, 1884, at 8 P.M.

Mr JAMES REID, President, in the Chair.

The Minute of General Meeting of 18th December, 1883, was read and approved, and signed by the President.

Mr J. H. BILES replied to the remarks upon his Paper on "The Stability of Ships at Launching," the discussion of which was terminated, and a vote of thanks awarded to Mr BILES for his Paper.

The discussion of Mr LAURENCE HILL'S Paper descriptive of an "Improved Railway Chair Key and Fish Joint," was resumed and terminated, and a vote of thanks was awarded Mr HILL for his Paper.

The discussion of Mr R. L. WEIGHTON'S Paper on "The Compound Engine Viewed in its Economical Aspect" took place and was continued to next General Meeting.

A Paper on "Cable Tramways," by Mr Ralph Moore, C.E., was read. A discussion followed and was continued to next General Meeting.

Diplomas of Membership were presented to gentlemen elected at previous General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows:—

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AS MEMBERS :—

Mr EDWARD HUNTER BUSHELL, Mechanical Engineer, Liverpool.

Mr J. Y. JOHNSON, Civil Engineer and Patent Agent, 115 St. Vincent Street.

Mr JAMES BROADFOOT, 55 Finnieston Street.
Mr WILLIAM BROWN, 61 Queen Street, Renfrew.
Mr W. M. COOKE, Gourlay Brothers &'Co., Dundee.
Mr PETER DEWAR, 25 North Street.
Mr BRUCE HARMAN, Messrs R. Napier & Sons, Govan.
Mr ROBERT WYLLIE, Hartlepool Engine Works, Hartlepool.

} previously Graduates.

AS GRADUATES :—

Mr GEORGE BLAIR, Jun., Apprentice Draughtsman, 6 Alfred Terrace, Hillhead.

Mr HENRY BLAIR, Apprentice Engineer, Clutha Iron Works.

Mr ALEX. M. COPELAND, Apprentice Engineer, Bellahouston Farm, Paisley Road.

Mr JAMES DALZIEL, Student Engineer, 119 Sandyford Street.

Mr WILLIAM DUNLOP, Mechanical Draughtsman, 961 Govan Road.

Mr THOMAS G. FERGUSON, Apprentice Engineer, 14 Queen's Crescent.

THE FIFTH GENERAL MEETING of the TWENTY-SEVENTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 26th February, 1884, at 8 P.M.

Mr JAMES REID, President, in the Chair.

The Minute of General Meeting of 22nd January, 1884, was read and approved, and signed by the President.

The Discussion of Mr R. L. WEIGHTON'S Paper on "The Compound Engine viewed in its Economical Aspect" was resumed and terminated, and a vote of thanks awarded Mr Weighton for his Paper.

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As the time did not permit of the discussion and reading of the other Papers as on Notice of Business these were deferred till next General Meeting.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows:—

AS MEMBERS:—

Mr JAMES T. COCHRAN, Mechanical Engineer, Duke St., Birkenhead.

Mr JOHN LANG, Jun., Engineer, Church Street, Johnstone.

Mr JOHN LIST, Superintendent Engineer, (Messrs D. Currie & Co., London).

Mr JAMES M'EWAN, Ironfounder, Cyclops Foundry, Glasgow.

Mr THOMAS D. WEIR, Civil Engineer, 97 West Regent St., Glasgow.

AS AN ASSOCIATE:—

Mr C. R. LEMKES, Representative of Messrs Schaffer & Budenberg, 198 Hope Street, Glasgow.

AS GRADUATES:—

Mr JOHN CLELAND, B.Sc., Assistant Engineer, Woodhead Cottage, Old Monkland.

Mr ARTHUR S. CLERK, Apprentice Engineer, 9 Carmichael St., Govan.

Mr ALEX. CONNER, Assistant Draughtsman, 9 Scott St., Glasgow.

Mr ALEX. GRACIE, Draughtsman, 9 Great George Street, Hillhead.

Mr ANDREW MUNRO, Ship Draughtsman, 629 Govan Road, Govan.

Mr D. J. NEVILL, Apprentice Draughtsman, 352 St. Vincent Street, Glasgow.

Mr MATTHEW PAUL, Jun., Apprentice Engineer, Levenford Works, Dumbarton.

Mr WALTER REID, Engineering Draughtsman, 90 Bellgrove Street, Glasgow.

Mr WM. WARRINGTON, Assistant C.E., 23 Miller Street, Glasgow.

Diplomas of Membership were presented to gentlemen elected at previous General Meeting.

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THE SIXTH GENERAL MEETING of the TWENTY-SEVENTH SESSION of the Institution was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 25th March, 1884, at 8 P.M.

Mr JAMES REID, President, in the Chair.

The Minute of General Meeting of 26th February, 1884, was read and approved, and signed by the President.

The Discussion of Mr RALPH MOORE'S Paper on "Cable Tramways" was resumed and terminated, and a vote of thanks awarded Mr Moore for his Paper.

The following Papers were read—"On Turbines," by Mr RICHARD MURRAY, and on "The Reliability of the Tests of Turbines," by Professor R. H. THURSTON. The Discussion of these Papers was deferred till next General Meeting.

Mr AND. MACLEAN and Mr DAVID KINGHORN were unanimously appointed Auditors of the Treasurer's Annual Financial Accounts.

The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows:—

AS MEMBERS:—

Mr JOHN HARVARD BILES, Naval Architect, Clydebank Shipyard.

Mr CHARLES CONNELL, Shipbuilder, Whiteinch.

Mr JOHN LAIDLAW, Mechanical Engineer, 98 Dundas Street, S.S., Glasgow.

AS GRADUATES:—

Mr J. B. SANDERSON, Apprentice Engineer, 15 India St., Glasgow.

Mr RUSSELL SINCLAIR, Marine Engineering Draughtsman, 9 Lawton Street, Newcastle-on-Tyne.

THE ANNUAL GENERAL MEETING of the INSTITUTION was held in the Hall of the Institution, 207 Bath Street, on Tuesday, the 22nd April, 1884, at 8 P.M.

Mr JAMES REID, President, in the Chair

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The Minute of General Meeting of 25th March, 1884, was read and approved and signed by the President.

The Treasurer's Annual Financial Statement, duly audited, was submitted to the Meeting, and on the motion of the President, was unanimously adopted, a vote of thanks being awarded to the Treasurer and Auditors.

The President announced that the Council had not been able to recommend the award of any medals for Papers read session 1882-83.

The Election of Office-Bearers then took place.

Professor JAMES THOMSON, C.E., LL.D., F.R.S., was unanimously elected President.

Mr T. ARTHUR ARROL was unanimously elected a Vice-President, and by a majority of votes the following gentlemen were elected Councillors:—Messrs ROBERT DUNDAS, WM. MACMILLAN, FRANK W. DICK, JAMES GILCHRIST, MATTHEW HOLMES, and ROBERT DUNCAN, Whitefield.

Mr JOHN THOMSON, of Messrs R. Laidlaw & Son, was unanimously re-elected as representative from the Institution to the College of Science and Arts.

The discussion of Professor THURSTON'S Paper was proceeded with and terminated, and a vote of thanks awarded the author of the Paper.

The Paper on "The Properties of Screw Piles," by Mr WILLIAM MURRAY, C.E., was held as read. A discussion took place and was continued till Opening Meeting of following Session.

Mr GEORGE SLIGHT, Jun., read his Paper on "Fog Signalling as Applied to Lighthouses," the discussion of which was delayed till Opening Meeting of following Session.

The paper on "Approximation to Curves of Stability from Data for Known Ships," by Messrs F. P. PURVIS and B. KINDERMANN, was held as read; the discussion of this paper was also delayed until the first meeting of the next Session.

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The President announced that the Candidates balloted for had been elected, the names of these gentlemen being as follows :—

AS HONORARY MEMBERS :—

Sir WILLIAM G. ARMSTRONG, C.B., LL.D., D.C.L., F.R.S., Newcastle-on-Tyne.

Professor H. HELMHOLTZ, Berlin.

Dr R. ANGUS SMITH, F.R.S., Manchester.

AS MEMBERS :—

Mr JOHN G. HUDSON, Mechanical Engineer, 18 Aytoun Road, Pollokshields.

Mr HARRY MAVOR, Electrical Engineer, 140 Douglas Street, Glasgow.

Mr R. A. ROBERTSON, Mechanical Engineer, 42 Aytoun Road, Pollokshields.

Mr ANDREW SCOTT, Mechanical Engineer, 154 West Regent Street.

AS A GRADUATE :—

Mr JOHN WEIR, Draughtsman, Messrs R. & W. Hawthorn, St. Peter's, near Newcastle-on-Tyne.

The President drew special attention to the Honorary Members elected as being gentlemen holding high and honourable positions in the engineering and scientific world, and congratulated the Institution on the cordiality shown by these gentlemen in their acceptance of the proposal made to them by the Council to become Honorary Members of the Institution.

On the motion of Mr J. M. GALE, a very hearty vote of thanks was awarded the President on his retirement from the Presidential Chair.

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DECEASED MEMBERS.

DURING the last Session the Institution has lost from its roll of Members several well-known gentlemen, who, from the beginning of the Institution, have taken a lively interest in its affairs, and some of whom have occupied the President's Chair.

Mr ROBERT BRUCE BELL was an original Member of the Institution, and at various times held office in the Council, and was President during Session 1876-78. He contributed to the Institution of his experience both by papers and remarks in discussion, and from his great energy of character took a strong and active interest in any movement likely to add to the prosperity of the Institution, and as a member of the Building Committee on the new rooms of the Institution he assisted actively in promoting the work to its successful issue.

Mr Bell's name is widely known as an engineer, and, in connection with Mr David Miller, the firm of Bell & Miller has contributed to the successful carrying out of many large and important engineering works, dock and harbour works, both at home and abroad. Of these we have well-known examples in our own neighbourhood, such as the Albert Bridge at Glasgow and the Albert Harbour and Esplanade, Greenock. Mr Bell some years ago was appointed commissioner to examine the river St. Lawrence for the improvement of the harbour of Montreal.

Mr CHARLES CONNELL was originally a member of the Scottish Shipbuilders' Association, founded in 1860, and incorporated with the Institution in 1865.

Mr Connell acted as a Member of Council of the Scottish Shipbuilders' Association from its commencement until it was incorporated with the Institution in 1865. Mr Connell's name is well-

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known in connection with shipbuilding, he having had great practical acquaintance with the design and construction of ships. The composite system of shipbuilding, in which the frames are of iron and outside sheathing of wood, was also carried out at Messrs Connell's works, some of the most successful sailing tea clippers being built on this system.

Mr J. L. K. JAMIESON joined the Institution in 1866, and acted at various times on the Council. In 1880 he was elected President, which office he held during Sessions 1880-81 and 1881-82. Mr Jamieson frequently contributed to remarks on discussion of papers, and took an active interest in all that concerned the welfare of the Institution. He was a member of the Committee on the New Buildings for the Institution, and was Chairman of the House Committee, besides acting frequently as a member of various Committees of Council. As a marine engineer he early gained experience at sea, and was afterwards superintendent abroad for the Pacific Steam Navigation Company. Latterly Mr Jamieson's name was well-known as a partner of the firm of Messrs John Elder & Co. Mr Jamieson took an active part in municipal affairs as a Town Councillor, and was also a director of railway and shipping companies and educational institutes.

Mr ANTHONY INGLIS was originally a member of the Scottish Shipbuilders' Association, holding the office of Vice-President of that association. He joined the Institution in 1865, when the Association was incorporated with the Institution. During Sessions 1866-67 and 1867-68 Mr Inglis held office as a Councillor of the united body of Engineers and Shipbuilders. Mr Inglis, besides being an active member of the well-known engineering and shipbuilding firm of Messrs A. & J. Inglis, interested himself in various local trusts and institutions.

Captain ALEXANDER BLACKWOOD was originally an associate of the Scottish Shipbuilders' Association, and joined the Institution in 1865, when the incorporation with the Institution took place.

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Captain Blackwood went early to sea, and had experience both in sailing and steam vessels. He joined the Cunard Service at its commencement, and was first officer of the "Britannia" on her maiden voyage across the Atlantic. During the last 40 years he has been connected with the Leith, Hull, and Hamburg Steam Packet Co.

Dr R. ANGUS SMITH, F.R.S., was recently elected an Honorary Member of the Institution. He was educated at Glasgow, and afterwards in Germany. In 1848 he presented a report to the British Association on "The Air and Water of Towns." He also investigated the condition of the air in mines and in mountainous districts, and carried out an inquiry on disinfectants. As a member of the Literary and Philosophical Society of Manchester (of which society he was at one time President) he contributed an interesting report published in the form of a volume entitled "A Centenary of Science in Manchester." Dr SMITH published a number of works all more or less bearing on chemical subjects.

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TREASURER'S STATEMENT—1883-84.

GENERAL FUND.

Dr.					Cr.		
To Balance in Union Bank at close of Session 1882-83,			£256	19 0	*By* Amount paid Treasurer of House Committee as Institution's proportion of Expenditure, for Session 1883-84, ...	£180	18 11
Subscriptions received:					Printing, ...	148	16 3
Session 1883-84, ..	£644	15 0			Lithography, ...	105	11 0
Arrears of Previous Sessions,	80	10 0			Institution Medal, Session 1882-83, ..	10	0 0
	£725	5 0			Graduate Section Medal, Session 1882-83, ...	1	7 6
Deduct Entry Money transferred to Building Fund, ..	14	15 0			Salary to Secretary, ...	125	0 0
			710	10 0	Commission Collection of Arrears of Subscriptions, viz.:—		
Sales of Transactions, ...			13	18 0	For Session 1883-84, ... £365 0 0		
Bank Interest, ...			2	9 3	For Previous Sessions, ... 80 10 0		
					£445 10 0 at 5%	22	5 6
					Postages, ...	32	12 0
					Delivery of Annual Volumes, ...	4	1 0
					Stationery, &c., ...	10	14 5
					New Books for Library, ...	8	10 0
					Cash to New Buildings Account to meet Interest on Loan, ...	16	0 0
					Petty Cash, ...	2	5 10
					Balance in Union Bank, ...	315	13 10
			£983	16 3		£983	16 3

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Dr. MARINE ENGINEERING MEDAL FUND. Cr.

To Balance in Union Bank at close of Session 1882-83, ...	£33	1	9	*By* Paid for Medal, Session 1882-83,... ..	£10	0	0
Interest on Capital lent to New Buildings Account, ...	10	0	0	Balance in Union Bank, ...	33	10	10
Bank Interest, ...	0	9	1				
	£43	10	10		£43	10	10

Dr. RAILWAY ENGINEERING MEDAL FUND. Cr.

To Balance in Union Bank at close of Session 1882-83, ...	£31	5	0	*By* Paid for Medal, Session 1882-83, ...	£10	0	0
Interest on Capital lent to New Buildings Account, ...	6	0	0	Balance in Union Bank, ...	27	13	8
Bank Interest, ...	0	8	8				
	£37	13	8		£37	13	8

Dr. GRADUATE MEDAL FUND. Cr.

To Balance in Union Bank at close of Session 1882-83,	£20	12	9	*By* Balance in Union Bank, ...	£21	2	3
Bank Interest, ...	0	9	6				
	£21	2	3		£21	2	3

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BUILDING FUND.

Dr.		Cr.	
To Balance in Union Bank at close of Session 1882-83,	£196 10 8	*By* Balance in Union Bank, ...	£234 6 0
One Life Member at £20, ...	20 0 0		
Entry Money, ...	14 15 0		
Bank Interest, ...	3 0 4		
	£234 6 0		£234 6 0

NEW BUILDINGS ACCOUNT.

Dr.			Cr.		
To Capital to meet Cost of New Buildings, viz.:—			*By* Paid on New Buildings to date, ...		£2,047 8 1
From General Fund, ...	£542 15 7		Interest on Loans, viz.:—		
,, Marine Engineering Medal Fund, ...	351 11 2		To Marine Engineering Medal Fund,	£10 0 0	
,, Railway Engineering Medal Fund, ...	213 13 3		,, Railway Engineering Medal Fund,	6 0 0	
,, Building Fund, ...	939 8 1				16 0 0
		£2,047 8 1			
Cash received from General Fund to meet Interest on Loans, ...		16 0 0			
		£2,063 8 1			£2,063 8 1

GLASGOW, *12th April, 1884.*—WE have examined the foregoing Annual Financial Statement of Treasurer, the Accounts of the Marine and Railway Engineering Medal Funds, the Graduate Medal Fund, the Building Fund, and the New Buildings Account, and find the same duly vouched and correct, the Amounts in Bank being as stated.

(Signed) ANDw. MACLEAN, D. KINGHORN, } AUDITORS.

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SUBSCRIPTION ACCOUNT.

Dr.					Cr.		
To Subscriptions due as per Roll:—					*By* Subscriptions received, as per Cash Book, viz.:—		
Arrears due at close of last Session, ...		£178 10 0			Arrears of Sessions previous to Session 1883-84, ...		£80 10 0
Deduct Irrecoverable, ...		39 0 0			SESSION 1883-84:—		
		£139 10 0			322 Members at £1 10 0 —	£483 0 0	
Add elected at Annual Meeting, 1883,		2 10 0			9 New Members ,, 2 10 0 —	22 10 0	
			£142 0 0		7 ,, ,, 2 0 0 —	14 0 0	
SESSION 1883-84:—					14 ,, ,, 1 10 0 —	21 0 0	
360 Members at £1 10 0 —	£540 0 0				33 Associates ,, 1 0 0 —	33 0 0	
9 New Members ,, 2 10 0 —	22 10 0				1 New Associate ,, 2 0 0 —	2 0 0	
7 ,, ,, 2 0 0 —	14 0 0				1 ,, ,, ,, 1 10 0 —	1 10 0	
15 ,, ,, 1 10 0 —	22 10 0				1 ,, ,, ,, 1 5 0 —	1 5 0	
36 Associates ,, 1 0 0 —	36 0 0				133 Graduates ,, 0 10 0 —	66 10 0	
1 New Associate ,, 2 0 0 —	2 0 0						644 15 0
1 ,, ,, ,, 1 10 0 —	1 10 0						£725 5 0
1 ,, ,, ,, 1 5 0 —	1 5 0				Arrears due for Session 1883-84, ...	£78 0 0	
166 Graduates ,, 0 10 0 —	83 0 0				Arrears due for previous Sessions,	61 10 0	
			722 15 0				139 10 0
			£864 15 0				£864 15 0

BANK ACCOUNT.

Dr.		Cr.	
To Balances at close of Session 1882-83:—		*By* Amounts Drawn, Session 1883-84, ...	£438 5 3
General Fund, ...	£256 19 0	Balances in Union Bank, ...	632 6 7
Marine Engineering Medal Fund, ...	33 1 9		
Railway Engineering Medal Fund, ...	31 5 0		
Graduate Medal Fund, ...	20 12 9		
Building Fund, ...	196 10 8		
Amounts lodged, Session 1883-84, ...	525 5 10		
Interest, Session 1883-84, ...	6 16 10		
	£1070 11 10		£1070 11 10

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CAPITAL ACCOUNT.

GENERAL FUND.

Loan to New Buildings Account, ...	£542 15 7		
Cash in Union Bank, ...	315 13 10		
		£858	9 5

MARINE ENGINEERING MEDAL FUND.

Loan to New Buildings Account, ...	£351 11 2		
Cash in Union Bank, ...	33 10 10		
		385	2 0

RAILWAY ENGINEERING MEDAL FUND.

Loan to New Buildings Account, ...	£213 13 3		
Cash in Union Bank, ...	27 13 8		
		241	6 11

GRADUATE MEDAL FUND.

Cash in Union Bank, ...		21	2 3

BUILDING FUND.

Amount to New Buildings Account, ...	939 8 1		
Cash in Union Bank, ...	234 6 0		
		1,173	14 1

ARREARS OF SUBSCRIPTIONS.

Arrears due for Session 1883-84.	£78 0 0		
Do. previous Sessions, ...	61 10 0		
		139	10 0
		£2,819	4 8

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DR. HOUSE EXPENDITURE ACCOUNT* (ABSTRACT 1883-84). CR.

To Rents for Letting Rooms, ...			£51	12	6	*By* Balance due Treasurer, ...	£38	17	9
Amounts received to meet Expenses, viz.:						Interest on Bond, ...	131	15	3
From Institution of Engineers and Shipbuilders, ...	£180	18 11				Salary to Curator, ...	90	0	0
From Philosophical Society,	144	0 5½				Expenses of Attendant at Library Cleaning, &c.,	24	16	4½
			324	19	4½	Taxes, ...	22	12	6
Balance due Treasurer, ...			4	1	5	Feu Duty (five years), ...	4	11	8
						Gas, ...	12	18	2
						Water, ...	7	13	4
						Coals, ...	6	14	0
						Insurance, ...	7	15	0
						Repairs, Painting, and Furnishing, ...	32	19	3
			£380	13	3½		£380	13	3½

* NOTE.—The Account of the House Committee is kept by Mr John Mann, C.A., Treasurer to the Committee, and is periodically audited by the Auditors appointed by the Institution and Philosophical Society.

W. J. MILLAR, Secretary to House Committee.

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DONATIONS TO LIBRARY.

Marine Boiler Construction, Corrosion, and Preservation, by John A. Rowe. From the Author.

Zur Theorie der Krafterbietragung durch Dynamo-electrisch Maschinen. Von R. Clausius. From the Author.

Lectures on the Application of Electricity, Institution of Civil Engineers. London, 1882-83. From the Institution.

A Centenary of Science in Manchester. Compiled by Dr R. Angus Smith, F.R.S. Presented by the Literary and Philosophical Society of Manchester.

ADDITIONS TO LIBRARY.

Practical Mechanics, - - -	Perry.
Steel and Iron, - - -	Greenwood.
Electricity and Magnetism, - -	Jenkin.
Bridges, - - - -	„
The Steam Engine, - - -	Cotterill.
Civil Engineering, - - -	Rankine.
Applied Mechanics, - - -	„
The Steam Engine, - - -	„
Machinery and Millwork, - -	„
Rules and Tables, - - -	„
Turbines, - - - -	Cullen. .
Tunnelling, - - - -	Gripper.
Water Supply, - - - -	Fanning.
Railway Appliances, - - -	Barry.
Strength of Materials, - - -	Anderson.
Properties of Metals, - - -	Bloxam.

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Theory of Heat, - - -	Clerk Maxwell.
Telegraphy, - - - -	Preece.
Workshop Appliances, - - -	Shelley.
Principles of Chemistry, - -	Tilden.
Elements of Machine Design, - -	Unwin.
Plane and Solid Geometry, - -	Watson.
The Study of Rocks, - - -	Rutley.
Marine Engineering, - - -	Sennet.
Mechanical Engineering, -	Knight.
Principles of Mechanics, - -	Goodeve.

THE INSTITUTION EXCHANGES TRANSACTIONS WITH THE FOLLOWING SOCIETIES:—

Institution of Civil Engineers.
Institution of Civil Engineers of Ireland.
Institution of Mechanical Engineers.
Institution of Naval Architects.
Institute of Mining and Mechanical Engineers.
Institute of Mining, Civil, and Mechanical Engineers.
Iron and Steel Institute.
Liverpool Polytechnic Society.
Literary and Philosophical Society of Manchester.
Mining Institute of Scotland.
Patent Office, London.
Philosophical Society of Glasgow.
Royal Scottish Society of Arts.
Royal Dublin Society.
South Wales Institute of Engineers.
Society of Engineers.
Society of Arts.
Association of Employers, Foremen, and Draughtsmen, Manchester.
American Society of Civil Engineers.
Geological Survey of Canada.
Smithsonian Institution, U.S.A.

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Stevens Institute of Technology, U.S.A.
Bureau of Steam Engineering, Navy Department, U.S.A.
Royal Society of Tasmania.
Royal Society of Victoria.
Royal Academy of Sciences, Lisbon.
Société des Ingénieurs Civils de France.
Société Industrielle de Mulhouse.
Société d'Encouragement pour l'Industrie Nationale.
Société des Anciens Elèves des Ecôles Nationales d'Arts et Métiers.
Société des Sciences Physiques et Naturelles de Bordeaux.
Austrian Engineers' and Architects' Society, Vienna.
Engineers and Architects' Society of Naples.
The Association of Civil Engineers of Belgium.
Master Car Builders' Association, U.S.A.

PUBLICATIONS RECEIVED PERIODICALLY IN EXCHANGE FOR INSTITUTION TRANSACTIONS:—

Annales Industrielles.
Annales de la Propriété Industrielle.
Colliery Guardian.
Engineering.
Iron.
Iron and Coal Trades' Review.
Journal de L'Ecole Polytechnic.
Mining Journal.
Nature.
Revue Industrielle.
The Engineer.
The Marine Engineer.
The American Manufacturer and Iron World.
The Contract Journal.

The Library of the Institution, at the Rooms, 207 Bath Street, is open daily from 9-30 a.m. till 8 p.m.; on Meeting Nights of the Institution and Philosophical Society, till 10 p.m.; and on Saturdays till 2 p.m. Books will be lent out on presentation of Membership Card to the Sub-Librarian.

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Members have also the privilege of consulting the Books in the Library of the Philosophical Society.

The use of Library and Reading Room is open to Members, Associates, and Graduates.

For Catalogue of Books in Library, see Vol. XII. of Transactions.

Copies of Catalogue and Supplement may be had from the Secretary.

The Portrait Album lies in the Library for the reception of Members' Portraits.

Members are requested when forwarding Portraits to attach Signature to bottom of Carte.

Members of this Institution, who may be temporarily resident in Edinburgh, will, on application to the Secretary of the Royal Scottish Society of Arts, at his Office, 117 George Street, be furnished with Billets for attending the Meetings of that Society.

The Meetings of the Royal Scottish Society of Arts are held on the 2nd and 4th Mondays of each Month, from November till April, with the exception of the 4th Monday of December.

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LIST

OF

HONORARY MEMBERS, MEMBERS, ASSOCIATES, AND GRADUATES

OF THE

Institution of Engineers and Shipbuilders in Scotland

(INCORPORATED),

SESSION 1883-84.

HONORARY MEMBERS.

JAMES PRESCOTT JOULE, LL.D., F.R.S., 12 Wardle Road, Sale, near Manchester.

Professor CHARLES PIAZZI SMYTH, F.R.S.E., Astronomer-Royal for Scotland, 15 Royal Terrace, Edinburgh.

Professor Sir WILLIAM THOMSON, A.M., LL.D., D.C.L., F.R.SS.L. and E., Professor of Natural Philosophy in the University of Glasgow.

Professor R. CLAUSIUS, the University, Bonn, Prussia.

Sir JOSEPH WHITWORTH, Bart., C.E., LL.D., F.R.S., Manchester.

Professor JOHN TYNDALL, D.C.L., LL D., F.R.S., &c., Royal Institution, London.

HIS GRACE THE DUKE OF SUTHERLAND, Trentham, Stoke-upon-Trent

Sir WM. G. ARMSTRONG, C.B., LL.D., D.C.L., F.R.S., Newcastle on-Tyne.

Professor H. VON HELMHOLTZ, Berlin.

Dr R. ANGUS SMITH, F.R.S., Manchester.

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MEMBERS.

DATE OF ELECTION.			
1883, Mar. 20:	Geo. A.	Agnew,	3 Gladstone Terrace, Govan.
1859, Jan. 19:	James	*Aitken, jun.,	Shipbuilder, Whiteinch, Glasgow.
1860, Dec 26:	William	Aiton,	Sandford Lodge, Peterhead.
Original:	William	Alexander,	23 India Street, Glasgow.
Original:	Alexander	Allan,	Glen House, The Valley, Scarborough.
1872, Feb. 27:	A. B.	Allan, C.E.,	Burgh Surveyor, Burgh Chambers, Govan.
1869, Jan. 20:	William	Allan,	Sunderland Engine Works, Sunderland.
1864, Dec. 21:	James B.	Alliott,	The Park, Nottingham.
G. 1865, Feb. 15: M. 1877, Dec. 18:	Wm. M.	Alston,	24 Burnbank Gardens, Glasgow.
1880, Apr. 27:	Michael	Altouhoff, C.E.,	26 Zagorodnoy Prospect, St. Petersburg, Russia.
1880, Nov. 23:	Thomas	Anderson,	Government Dockyard, Bombay.
G. 1874, Feb. 24: M. 1880, Nov. 23:	James	Anderson,	100 Clyde St., Glasgow.
1860, Nov. 28:	Robert	Angus,	Lugar Ironworks, Cumnock.
1883, Dec. 18:	J. Cameron	Arrol,	18 Blythswood Square, Glasgow.
1875, Dec. 21:	Thomas A. (*Member of Council.*)	Arrol,	18 Blythswood Square, Glasgow.
Original:	David	Auld,	65 Rochester St., Glasgow.
1881, Oct. 25:	Allan W.	Baird,	100 Paisley Road W., Glasgow
1880, Feb. 24:	William N.	Bain,	East Point, Hong Kong, China.
1873, Apr. 22:	H. W.	Ball,	Cranstonhill Engine Works, Glasgow.

Names marked thus * were Members of Scottish Shipbuilders' Association at Incorporation with Institution, 1865.

Names marked thus † are Life Members.

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1858, Dec. 22: Andrew Barclay, F.R.A.S., Caledonian Foundry, Kilmarnock.

1876, Jan. 25: James Barr, London Road Iron Works, Glasgow.

1882, Mar. 21: Prof. Archd. Barr, B.Sc., C.E., The Yorkshire College, Leeds.

1868, Apr. 22: Edward Barrow, Manager of Bureau Veritas, 11 Rue de Phalsburg, Paris.

1881, Mar. 22: George H. Baxter, Ramage & Ferguson, Leith.

1875, Jan. 26: Charles Bell, 4 Clifton Place, Glasgow.

David *Bell, Shipbuilder, Yoker, near Glasgow.

1868, Feb. 12: Edward M. Bell, TinplateWorks, Coatbridge.

1880, Mar. 23: Imrie Bell, C.E., 1 Victoria Street, Westminster, London, S.W.

1880, Nov. 2: Alfred G. Berry, 33 Carnarvon St., Glasgow.

1884, Mar. 25: John Harvard Biles, Clydebank Shipyard, near Glasgow.

Neil *Black, 68 Gt. Howard St., Liverpool.

1866, Dec. 26: Edward Blackmore, Eagle Foundry, Greenock.

1864, Oct. 26: Thomas Blackwood, Shipbuilder, Port-Glasgow.

1869, Feb. 17: Geo. M'L. Blair, 127 Trongate, Glasgow.

1867, Mar. 27: James M. Blair, 38 Elmbank Cres., Glasgow.

1883, Jan. 23: Chas. C. Bone, C.E, 23 Miller Street, Glasgow.

1883, Oct. 23: William L. Bone, Ant and Bee Works, West Gorton, Manchester.

1874, Jan. 27: Howard Bowser, 13 Royal Crescent, W. Glasgow.

1880, Mar. 23: James Brand, C.E., 109 Bath Street, Glasgow.

G. 1873, Dec. 23: } James Broadfoot, 55 Finnieston St., Glasgow.
M. 1884, Jan. 22: }

1865, Apr. 26: Walter *Brock, Engine Works, Dumbarton.

1859, Feb. 16: Andrew *Brown, London Works, Renfrew.

1880, Dec. 21: William Brown, Albion Works, Woodville Street, Govan, Glasgow.

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G. 1874, Jan. 27: M. 1884, Jan. 22:	William	Brown,	61 Queen Street, Renfrew.
1858, Mar. 17:	James	Brownlee,	23 Burnbank Gardens, Glasgow.
1877, Oct. 30:	Robert	Bruce,	12 Kelvingrove St., Glasgow.
1860, Dec. 26:	James C.	Bunten,	100 Cheapside St., Glasgow.
1866, Apr. 26:	Amedee	Buquet, C.E.,	15 Chemiss, St. Martin. Pontoise, S. O. France.
	Andrew	*Burns,	Hilton of Burleigh, Milnathort.
1880, Dec. 21:	James W.	Burns,	Caird, Purdie, & Co., Barrow-in-Furness.
1881, Mar. 22:	Thomas	Burt,	371 New City Rd., Glasgow.
1884, Jan. 22:	Edward Hunter	Bushell,	G 19 Exchange Buildings, Liverpool.
1878, Oct. 29:	Edward B.	Caird,	8 Scotland St., Glasgow.
1878, Dec. 17:	James	Caldwell,	130 Elliot Street, Glasgow.
1875, Dec. 21:	J. C.	Cameron,	24 Pollok Street, Glasgow.
1880, Dec. 21:	Alexander	Campbell, jun.,	155 North Street, Glasgow.
1868, Dec. 23:	David	Carmichael,	Ward Foundry, Dundee.
1859, Nov. 23:	Peter	Carmichael,	Dens Works, Dundee.
1862, Jan. 8:	John	Carrick,	6 Park Quadrant, Glasgow.
1881, Nov. 22:	John H.	Carruthers,	46 Langside Rd., Glasgow.
1859, Oct. 26:	Robert	Cassels,	168 St. Vincent Street, Glasgow.
1867, Jan. 30:	Albert	Castel,	3 Lombard Court, London, E.C.
G. 1873, Dec. 23: M. 1883, Oct. 23:	Walter	Chambers,	Ulster Chambers, Belfast.
1882, Mar. 21:	Robert	Clachar,	25 St. Vincent Cres., Glasgow.
1883, Jan. 23:	John	Clark,	British India Steam Navigation Co., Calcutta.
1875, Oct. 26:	W. J.	Clark,	Southwick, near Sunderland.
1880, Nov. 2:	James	Clarkson,	Maryhill Engine Works, Maryhill, Glasgow.
1860, Apr. 11:	James	Clinkskill,	1 Holland Place, Glasgow.

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1884, Feb. 26:	James T.	Cochran,	Duke Street, Birkenhead.
1881, Oct. 25:	George	Cockburn,	2 Taylor Pl., Govan Road.
G. 1876, Dec. 19: M. 1884, Mar. 25:	Charles	Connell,	Whiteinch, Glasgow.
	Charles	Connell,	Whiteinch, Glasgow.
Original:	Robert	Cook,	Woodbine Cottage, Pollokshields, Glasgow.
G. 1876, Jan. 25: M. 1884, Jan. 22:	William M.	Cooke,	Gourly Bros. & Co., Dundee.
1864, Feb. 17:	James	Copeland,	16 Pulteney St., Glasgow.
1864, Jan. 20:	William R.	Copland, C.E.,	146 West Regent Street Glasgow.
1868, Mar. 11:	S. G. G.	Copestake, (*Vice-President.*)	Glasgow Locomotive Works, Little Govan, Glasgow.
1866, Nov. 28:	M'Taggart	Cowan, C.E.,	109 Bath Street, Glasgow.
1868, Apr. 22:	David	Cowan, C.E.,	Mount Gerald House, Falkirk.
1861, Dec. 11:	William	Cowan,	Great North of Scotland Railway, Aberdeen.
1883, Dec. 18:	Samuel	Crawford,	Clydebank, near Glasgow.
1881, Mar. 22:	William	Crockatt,	2 Marjory Place, Pollokshields, Glasgow.
1866, Dec. 26:	James L.	Cunliff,	Plewlands House, Merchiston, Edinburgh.
1872, Nov. 26:	David	Cunningham,	C.E., Harbour Chambers, Dundee.
1869, Jan. 20:	James	Currie,	16 Bernard Street, Leith.
G. 1874, Feb. 24: M. 1882, Dec. 19:	James	Davie,	1 Kelburn Terrace, Crosshill, Glasgow.
1861, Dec. 11:	Thomas	Davison,	248 Bath Street, Glasgow.
1864, Feb. 17:	St. J. V.	Day, C.E.,	115 St. Vincent St., Glasgow.
1869, Feb. 17:	James	Deas, C.E.,	Engineer, Clyde Trust, 7 Crown Gardens, Glasgow.
1882, Dec. 19:	J. H. L. Van	Deinse,	31 Prins Hendrikkade, Amsterdam.
1883, Nov. 21:	James	Denholm,	360 Dumbarton Road, Glasgow.

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1866, Feb. 14:	A. C. H.	Dekke,	Shipbuilder, Bergen, Norway.
	Peter	*Denny,	Helenslee, Dumbarton.
1873, Feb. 18:	William	Denny,	Leven Shipy'd, Dumbarton.
	(*Member of Council.*)		
G. 1873, Dec. 23: M. 1884, Jan. 22:	Peter	Dewar,	25 North Street, Glasgow.
1878, Mar. 19:	Frank W.	Dick,	405 Eglinton Street, Glasgow.
G. 1873, Dec. 24: M. 1878, Jan. 22:	James S.	Dixon,	170 Hope Street, Glasgow.
1882, Nov. 28:	John G.	Dobbie,	British India Steam Navigation Co., Mazagon Dockyard, Bombay.
1871, Jan. 17:	William	Dobson,	The Chesters, Jesmond, Newcastle-on-Tyne
1864, Jan. 20:	James	Donald,	Abbey Works, Paisley.
1876, Jan. 25:	James	Donaldson,	Fulbar Street, Renfrew.
1863, Nov. 25:	Robert	Douglas,	Dunnikier Foundry, Kirkcaldy.
1883, Feb. 20:	Thomas	Downie,	1 Scotland St, S.S., Glasgow.
1882, Oct. 24:	Chas. R.	Dubs,	Glasgow Locomotive Works, Glasgow.
1864, Oct. 26:	Robert	*Duncan,	Shipbuilder, Port-Glasgow.
	(*Past President; Vice-President.*)		
1881, Jan. 25:	Robert	Duncan,	Whitefield Engine Works, Glasgow.
1873, Apr. 22:	Robert	Dundas, C.E.,	3 Germiston Street, Glasgow.
1869, Nov. 23:	David Jno.	Dunlop,	Inch Works, Port-Glasgow.
1877, Jan. 23:	John G.	Dunlop,	17 Goulton Road, Lower Clapton, London.
1880, Mar. 23:	Hugh S.	Dunn,	Earlston Villa, Caprington, Kilmarnock.
1879, Dec. 23:	Wm. T.	Courtier Dutton	30 Gordon Street, Glasgow.
1883, Oct. 23:	Prof. Henry	Dyer, C.E.,	Dunrowan, Lenzie, near Glasgow.

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1876, Oct. 24: John Marshall Easton, Redholm, Helensburgh.

1875, Oct. 26: James G. Fairweather, C.E., B.Sc., 8 Findhorn Place, Grange, Edinburgh.

John *Ferguson, Shipbuilder, Whiteinch, Glasgow.

G. 1869, Nov. 23: } John Ferguson, jun., Shipbuilder, Leith.
M. 1878, Mar. 19: }

1874, Feb. 24: Immer Fielden, 47 Queen's Square, Belfast.

1880, Jan. 27: Alexander Findlay, °/o James Goodwin & Co., Motherwell.

Original: William Forrest, 77 Renfield St., Glasgow.

1872, Nov. 26: Thomas Forrest, M.E., Dumfries Ironworks, Dumfries.

1883, Dec. 18: Lawson Forsyth, 10 Grafton Sq., Glasgow.

1870, Jan. 18: William Foulis, (*Member of Council.*) Engineer, Corporation Gas Works, 42 Virginia St., Glasgow.

1880 Nov. 2: Samson Fox, Leeds Forge, Leeds.

1862, Nov. 26: Alexander Fullarton, Vulcan Works, Paisley.

1879, Nov. 25: John Frazer, P. Henderson & Co., 15 St. Vincent Place, Glasgow.

1858, Nov. 24: James M. Gale, C.E., (*Past President; Member of Council, and Treasurer.*) Engineer, Corporation Water Works, 23 Miller Street, Glasgow.

1862, Jan. 8: Andrew Galloway, C.E., St. Enoch Station, Glasgow.

1883, Oct. 23: Gilbert H. Garret, 47 West Cumberland St., Glasgow.

1873, Dec. 23: Bernard Gatow, Veritas Office, 29 Waterloo Street, Glasgow.

G. 1873, Dec. 23: } Andrew Gibb, Rait & Gardiner, Millwall
M. 1882, Mar. 21: } Docks, London.

1859, Nov. 23: Archibald *Gilchrist, 11 Sandyford Pl., Glasgow,

G. 1866, Dec. 26: } James Gilchrist, Stobcross Engine Works,
M. 1878, Oct. 29: } Finnieston Quay, Glasgow.

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1859, Dec. 21: David C.	Glen,	14 Annfield Place, Dennistoun, Glasgow.
1868, Nov. 25: Thomas	Goldie,	Post Office, Cape Town, Cape of Good Hope.
1864, Feb. 17: James	Goodwin,	Ironfounder, Ardrossan.
1866, Mar. 28: Gilbert S.	Goodwin,	Alexandra Buildings, James Street, Liverpool.
1868, Mar. 11: Joseph	Goodfellow,	136 Sackville Place, Stirling Road, Glasgow.
1858, Dec. 22: Henry	*Gourlay,	Dundee Foundry, Dundee.
1882, Apr. 25: H. Garrett	Gourlay,	Dundee Foundry, Dundee.
Edwin	*Graham,	Osbourne, Graham, & Co., Hylton, Sunderland.
1858, Mar. 12: George	Graham, C.E.,	Engineer, Caledonian Railway, Glasgow
1876, Jan. 25: Thomas M.	Grant,	Townholm Engine Works, Kilmarnock
1869, Feb. 17: Alex. D.	Gray,	Moore Park Boiler Works, Helen Street, Govan.
1871, Mar. 28: Thomas	Gray,	Chapel Colliery, Newmains.
1861, Dec. 11: Archibald	Gray,	Middleton House, Dalry.
1862, Jan. 8: James	Gray,	Pathhead Colliery, Cumnock, Ayrshire
1867, Nov. 27: James	Grier,	29 Waterloo St., Glasgow.
1870, Feb. 22: P. B. W.	Gross, M.E.,	4 Albion Place, Cumberland Road, Bristol.
1881, Dec. 20: L. John	Groves,	131 Hope Street, Glasgow.
1879, Nov. 25: Robert	†Hadfield,	Hadfield Steel Foundry Co., Attercliffe, Sheffield.
1872, Feb. 27: A. A.	Haddin, C.E.,	131 West Regent Street, Glasgow.
1881, Jan. 25: William	Hall, jun.,	Shipbuilder, Aberdeen.
1876, Oct. 24: David	Halley, (*Member of Council.*)	Burmeister & Wain, Copenhagen, Denmark.

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G. 1873, Dec. 23: M. 1881, Nov. 22:	David C.	Hamilton, (*Member of Council.*)	Clyde Shipping Co., 21 Carlton Place, Glasgow.
	James	*Hamilton,	7 Queen's Gardens, Dowanhill, Glasgow.
G. 1866, Dec. 26: M. 1873, Mar. 18:	James	Hamilton, jn., (*Member of Council.*)	Ardedynn, Kelvinside, Glasgow.
	John	*Hamilton,	22 Athole Gardens, Gl'gow.
G. 1869, Nov. 23: M. 1875, Feb. 23	J. B.	Hamond,	The Victoria Engineering Company, Victoria Works, Stockport.
1876, Feb. 22:	Walter	Hannah,	Board of Trade Surveyor, 7 York Street, Glasgow.
G. 1880, Nov. 2: M. 1884, Jan. 22:	Bruce	Harman,	R. Napier & Sons, Govan, Glasgow.
1878, Mar. 19:	Timothy	Harrington,	61 Gracechurch Street, London, E.C.
1875, Jan. 26:	Peter T.	Harris,	19 West St. (S.S.), Glasgow.
G. 1874, Feb. 24: M. 1880, Nov. 23.	C. B.	Harvey,	166 Renfrew St., Glasgow.
1864, Nov. 23:	John	Hastie,	Kilblain Engine Works, Greenock.
1871, Jan. 17:	William	Hastie,	Kilblain Engine Works, Greenock.
1879, Nov. 25:	A. P.	†Henderson,	30 Lancefield Quay, Glasgow.
1877, Feb. 20:	David	*Henderson,	Meadowside, Partick, Glasgow.
1873, Jan. 21:	John	†Henderson, jun.,	Meadowside, Partick, Glasgow.
1879, Nov. 25:	John L.	†Henderson,	Westbank House, Partick, Glasgow.
1878, Dec. 17:	William	Henderson,	Meadowside, Partick, Glasgow.
1875, Jan. 26:	William	Henderson,	61 Bishop St., Anderston, Glasgow.
1880, Nov. 2:	William	Henderson, C.E.,	121 W. Regent St., Gl'gow.
1870, May 31:	Richard	Henigan, C.E.,	Alma Terrace, Avenue Road, Southampton.

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1877, Feb. 20:	George Laurence	Herriot, *Hill, C.E.,	7 York Street, Glasgow. 59 St. Vincent St., Glasgow.
1880, Nov. 2:	C. P.	Hogg, C.E., (*Member of Council.*)	175 Hope Street, Glasgow.
1883, Mar. 20:	John	Hogg,	Victoria Engine Works, Airdrie.
1880, Mar. 23:	F. G.	Holmes, C.E.,	103 Bath Street, Glasgow.
1883, Mar. 20:	Matthew	Holmes,	551 Sauchiehall St., Glasgow.
Original:	James	Howden,	8 Scotland Street, Glasgow.
1884, Apr. 22:	John G.	Hudson,	18 Aytoun Road., Pollokshields, Glasgow.
Original:	Edmund	*Hunt,	87 St. Vincent Street, Glasgow.
1860, Nov. 28:	James	Hunter,	Coltness Iron Works, by Newmains.
1881, Jan. 25:	James	Hunter,	Aberdeen Iron Works, Aberdeen.
1857, Dec. 23:	John	Hunter,	Dalmellington Iron Works, near Ayr.
G. 1873, Dec. 23: M. 1877, Feb. 20:	P. S.	Hyslop, M.I.C.E.,	Buenos Ayres.
	Anthony	*Inglis,	64 Warroch Street, Glasgow.
Original:	John	*Inglis,	64 Warroch Street, Glasgow.
1861, May 1:	John	Inglis, junr.,	Point House Shipyard, Glasgow.
1879, Jan. 21:	Thos. F.	Irwin,	30 Old Churchyard, Liverpool.
1880, Nov. 2:	Lawrence N.	Jackson,	Engineer, Railway Works, Colombo, Ceylon.
1875, Dec. 21:	William	Jackson,	Govan Engine Works, Govan, Glasgow.
	Geo. W.	*Jaffrey,	17 Robertson St., Greenock.
1884, Jan. 22:	J. Yate	Johnson, C.E.,	115 St. Vincent Street, Glasgow.

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1879, Feb. 25:	David	Johnstone,	7 Carmichael St., Govan, Glasgow.
1870, Dec. 20:	David	Jones,	Highland Rlwy., Inverness.
1883, Jan. 23:	F. C.	Kelson,	Greenbank, Waterloo, Liverpool.
1872, Mar. 26:	Ebenezer	Kemp,	Linthouse Engine Works, Govan, Glasgow.
1875, Nov. 23:	William	Kemp,	Ellen St. Engineering Works, Govan, Glasgow.
1878, Mar. 19:	Hugh	Kennedy,	Redclyffe, Partickhill, Glasgow.
1877, Jan. 23:	John	Kennedy,	Bond Court Chambers, Walbrook, London, E.C.
1876, Feb. 22:	Thomas	Kennedy,	Water Meter Works, Kilmarnock.
1876, Oct. 24:	Andrew	Kerr, C.E.,	Warrnambool, Victoria, Australia.
1876, Mar. 21:	John G.	Kincaid,	Clyde Foundry, Greenock.
	David	*Kinghorn,	172 Lancefield St., Glasgow.
1879, Dec. 23:	John G.	Kinghorn,	2 Alexandra Terrace, Rock Ferry, Cheshire.
1864, Oct. 26:	Alex. C.	Kirk,	19 Athole Gardens, Hillhead, Glasgow.
Original:	David	*Kirkaldy,	Testing and Experimenting Works, Southwark St., London, S.E.
1880, Mar. 23:	Frederick	Krebs,	M.B.M.S.S. Co., Tokio, Japan.
1875, Oct. 26:	William	Laing,	72 Gt. Clyde St., Glasgow.
1858, Apr. 14:	David	Laidlaw,	Chaseley, Skelmorlie, by Glasgow.
1884, Mar. 25:	John	Laidlaw,	98 Dundas St., S.S., Gl'gow.
1862, Nov. 26:	Robert	Laidlaw,	147 E. Milton St., Glasgow,
1880, Feb. 24:	James	Lang,	Superintendent Engineer, Kobe, Japan.
1884, Feb. 26:	John	Lang, Jun.,	Church Street, Johnstone.

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Original:	James G.	*Lawrie, (*Past President.*)	2 Westbourne Terrace, Glasgow.
1882, Mar 21:	Henry Adamson	Lawson,	Craigenly Cottage, Lenzie, near Glasgow.
	Andrew	*Leckie,	Surveyor, Commercial Buildings, West Hartlepool.
1880, Mar. 23:	Allison	Lennox,	131 W. Regent St., Glasgow.
1878, Mar. 19:	John	Lennox,	131 W. Regent St., Glasgow.
G. 1873, Dec. 23: } M. 1876, Oct. 24: }	Charles C.	Lindsay, C.E., (*Vice-President.*)	167 St. Vincent St., Glasgow.
1884, Feb. 26:	John	List,	Messrs D. Currie & Co., London.
1862, Apr. 2:	H. C.	Lobnitz,	Renfrew.
1865, Dec. 20:	John L.	Lumsden,	Alex. Jack & Coy., Seacombe, Cheshire.
	James	*Lyall,	13A Exchange Buildings, Liverpool.
1873, Jan. 21:	James M.	Lyon, M.E.,	Engineer and Contractor, Singapore.
1862, Oct. 29:	John	M'Andrew,	17 Park St. East, Glasgow.
1858, Feb. 17:	David	M'Call, C.E.,	160 Hope Street, Glasgow.
1874, Mar. 24:	Hector	MacColl,	Jas. Jack & Co., Engineers, Liverpool.
	Hugh	*MacColl,	Manager, Wear Dock Yard, Sunderland.
1883, Oct. 23:	James	M'Creath, C.E.,	95 Bath Street, Glasgow.
1871, Jan. 17:	David	M'Culloch,	Vulcan Works, Kilmarnock.
1857, Dec. 23:	J. I.	M'Derment,	41 New Market Street, Ayr.
1884, Feb. 26:	James	M'Ewan,	Cyclops Foundry, Glasgow.
1880, Nov. 2:	James W.	Macfarlane,	18 Windsor St., Glasgow.
Original:	Walter	MacFarlane,	Possil Park, Glasgow.
	Andrew	*M'Geachan,	Newark Shipbuilding Yard, Port-Glasgow.
1880, Apr. 27:	Wm. Rae	M'Kaig,	17 Water St., Liverpool.

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1881, Mar. 22:	William A.	Mackie,	3 Broomhill Terrace, Partick, Glasgow.
1873, Jan. 21:	J.B. Affleck	M'Kinnel,	Dumfries Iron Works, Dumfries.
1859, Dec 21:	Robert	M'Laren,	22 Canal St., S.S., Glasgow.
	Andrew	*Maclean,	Viewfield House, Partick, Glasgow.
1858, Nov. 24:	Walter	M'Lellan,	127 Trongate, Glasgow.
	John	*M'Millan,	Shipbuilder, Dumbarton.
	William	*MacMillan,	19 Elgin Terrace, Partick. Glasgow.
Original:	Andrew	M'Onie,	1 Scotland Street, Glasgow.
1883, Jan. 23:	William	M'Onie, Jr.,	128 West Street, Glasgow.
1883, Jan. 23:	James	M'Ritchie, C.E,	Singapore.
1864, Oct. 26:	Robert	*Mansel, (*Past President.*)	Shipbuilder, Whiteinch, Glasgow.
1875, Dec. 21:	George	Mathewson,	Bothwell W'ks, Dunfermline.
1884, Apr. 22:	Harry	Mavor,	140 Douglas St., Glasgow.
1876, Jan. 25:	William W.	May.	142 Fountain Road, Walton, Liverpool.
1883, Feb. 20:	James	Meek,	10 Clarence Road, Devonshire Park, Birkenhead.
G. 1876, Oct. 24: / M. 1882, Nov. 28:	James	Meldrum, C.E.,	3 Elmbank Street, Glasgow.
1883, Jan. 23:	William	Melville, C.E.,	Caledonian Ry., Buchanan Street, Glasgow.
1881, Mar. 22:	William	Menzies,	7 Dean Street, Newcastle-on-Tyne.
1861, Dec. 11:	Daniel	Miller, C.E.,	204 St. Vincent St., Glasgow.
	James	*Miller,	Kelvin Forge, Partick, Glasgow.
G. 1873, Dec. 23: / M. 1881, Nov. 22:	John F.	Miller,	204 Stobcross St., Glasgow.
1875, Oct. 26:	Leighton	Mills,	Principal Board of Trade Officer, N. E. District, 1 S. Preston, N. Shields
Original:	James B.	Mirrlees,	45 Scotland St., Glasgow.

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1876, Mar. 21:	James	Mollison,	Lloyd's Register, 36 Oswald Street, Glasgow.
1869, Dec. 21:	John	Montgomerie,	Caledonian Railway Works, Perth.
1883, Nov. 21:	Joseph	Moore,	Risdon Foundry, San Francisco.
1862, Nov. 26:	Ralph	Moore, C.E.,	Croft Villa, Rutherglen.
1878, Apr. 23:	Robert H.	Moore,	Smeaton Park, Inveresk.
1868, Feb. 12:	Alexander	Morton,	241 W. George St., Glasgow.
G. 1878, Dec. 17: M. 1883, Jan. 23	Robert	Morton,	53 Waterloo St., Glasgow
1864, Feb. 17:	Hugh	Muir.	345 Bath Crescent, Glasgow.
1882, Jan. 24:	John G.	†Muir,	100 Cheapside St., Glasgow.
1870, Mar. 22:	Wm. T.	Mumford,	36 Oswald Street, Glasgow.
1882, Feb. 21:	George	Munro,	254 Bath Street, Glasgow.
1882, Dec. 19:	Robert	Munro,	162 Buchanan St., Glasgow.
Original:	James	Murdoch,	Shipbuilder, Port-Glasgow.
1880, Jan, 27:	William	Murdoch,	20 Carlton Place, Glasgow.
1874, Mar. 24:	Richard	Murray,	160 Hope Street, Glasgow.
1877, Jan. 23:	Robert	Murray,	25A Coltman Street, Hull.
1881, Jan. 25:	Henry M.	Napier,	Shipbuilder, Yoker, near Glasgow.
1857, Dec. 23:	John	†*Napier.	23 Portman Sq., London.
1881, Dec. 20:	Robert T.	†Napier,	Shipbuilder, Yoker, near Glasgow.
Original:	Walter	Neilson,	172 West George Street, Glasgow.
Original:	Walter M. (*Past President.*)	Neilson,	Queen's Hill, Kirkcudbrightshire.
1869, Nov. 23:	Theod. L.	Neish,	78 Finnart St., Greenock.
A. 1865, Apr. 26: M. 1879, Oct. 28:	R. S.	*Newall, F.R.S., F.R.A.S., &c.,	Ferndene, Gateshead on-Tyne.
	Benjamin	*Nicholson,	Annan.
1883, Dec. 18.	Thomas	Nicol,	Clydebank, near Glasgow.
1876, Dec. 19:	Richard	Niven, C.E.,	Dalnottar House, Old Kilpatrick.

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1861, Dec. 11: John	Norman,	475 New Keppochhill Road, Glasgow.
1882, Jan. 24: Robert S.	Oliver, C.E.,	Highland Railway Co., Inverness.
1860, Nov. 28: John W.	Ormiston,	Shotts Iron Works, by Wishaw.
1867, Apr. 24: T. R.	Oswald,	The Southampton Shipbuilding & Engineering Works, Southampton.
1882, Mar. 21: Geo. S.	Packer, F.I.C.,	Hallside Steel Works, Newton, near Glasgow.
1864, Oct. 26: John	Page, C.E.,	1 Kersland Ter., Glasgow.
1876, Apr. 25: William	Parker,	2 White Lion Court, Cornhill, London.
1883, Nov. 21: W. L. C.	Paterson,	19 St. Vincent Crescent, Glasgow.
1877, Apr. 24: Andrew	Paul,	Levenford Works, Dumbarton.
1880, Nov. 2: James M.	Pearson, C.E.,	8 Duke St., Kilmarnock.
1866, Dec. 26: William	Pearce,	Fairfield Shipyard, Govan, Glasgow.
1868, Dec. 23: Eugène	Perignon, C.E.,	105 Rue Faubourg, St. Honoré, Paris.
John	*Price,	Rose Villa, Gateshead Road, Jarrow-on-Tyne.
1877, Nov. 20: F. P.	Purvis,	14 Strathleven Place, Dumbarton.
1868, Dec. 23: Henry M.	Rait,	155 Fenchurch St., London.
1873, Apr. 22: Richard	Ramage,	Shipbuilder, Leith.
1866, Dec. 26: Daniel	Rankin,	Eagle Foundry, Greenock.
1872, Oct. 22: David	Rankine,	75 West Nile St., Glasgow.
1876, Dec. 19: Robert	Rankin,	35 Paisley Road, Glasgow.
1881, Jan. 25: Charles	Reid,	Lilymount, Kilmarnock.
1883, Nov. 21: George W.	Reid,	Highland Rly., Inverness.
1868, Mar. 11: James	Reid, (*President.*)	Locomotive Works, Springburn, Glasgow.

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1869, Mar. 17:	James,	Reid,	Shipbuilder, Port-Glasgow.
	John	*Reid,	Shipbuilder, Port-Glasgow.
1880, Apr. 27:	John	Rennie,	Marylea Villa, Busby.
G. 1873, Dec. 23: M. 1881, Nov. 22:	Charles H.	Reynolds,	Cuprum House, Hamilton Ter., Partick, Glasgow.
1876, Oct. 24:	Duncan	Robertson,	8 Brighton Place, Govan, Glasgow.
Original:	James	Robertson,	21 Gower Street, Paisley Road, Glasgow.
1873, Jan. 21:	John	Robertson,	Grange Knowe, Pollokshields, Glasgow.
1873, Apr. 22:	M. W.	Robertson,	Avon Steel Works, Duke Street, Glasgow.
1863, Nov. 25:	William	Robertson, C.E.,	123 St. Vincent Street, Glasgow.
1884, Apr. 22:	R. A.	Robertson,	42 Aytoun Road, Pollokshields, Glasgow.
Original:	Hazltn. R.	*Robson, (*Past President.*)	14 Royal Cresct., Glasgow.
1877, Feb. 20:	Jno. Macdonald	Ross,	11 Queen's Cres., Glasgow.
1861, Dec. 11:	Richard G.	Ross,	21 Greenhead St., Glasgow.
G. 1864, Nov. 23: M. 1870, Jan. 18:	Alex.	Ross, C.E.,	Alva.
Original:	David	*Rowan, (*Past President.*)	231 Elliot Street, Glasgow.
G. 1858, Dec. 22: M. 1863, Mar. 4:	George	Russell,	Engineer, Motherwell.
1877, Oct. 30:	Alexander	Russell,	186 North Street, Glasgow.
1859, Dec. 21:	Thomas	*Russell,	Albyn Lodge, Bridge of Allan.
1881, Feb. 22:	Joseph	Russell,	Shipbuilder, Port-Glasgow.
1876, Oct. 24:	Peter	Samson,	Board of Trade Offices, Downing St., London, S.W.
1883, Feb. 20:	John	Sanderson,	Lloyd's Registry, 30 Oswald Street, Glasgow.
1882, Dec. 19:	Prof. Jas.	Scorgie, F.C.S.,	Civil Engineering College, Poona, India.

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1884, Apr. 22: Andrew	Scott,	154 W. Regent St., Glasgow.
1872, Jan. 30: James E.	Scott,	13 Rood Lane, London.
1881, Jan. 25: John	Scott,	Aberdeen Iron Works, Aberdeen.
1860, Nov. 28: Thos. B.	*Seath,	42 Broomielaw, Glasgow.
1875, Jan. 26: Alexander	Shanks,	Belgrade, Ayton Road, Polokshields, Glasgow.
1858, Nov. 24: William	Simons,	Renfrew.
1862, Jan. 22: Alexander	Simpson, C.E.,	175 Hope Street, Glasgow.
1871, Mar. 28: Hugh	Smellie,	Belmont Grange Terrace, Kilmarnock.
Original: Alexander	Smith,	57 Cook Street, Glasgow.
1880, Nov. 2: Alexander	Smith,	1 Braeside Terrace, Maxwell Rd., Pollokshields, Glasgow.
1869, Mar. 17: David S.	Smith,	Hellenic Steam Navigation Co., Syra, Greece.
1859, Jan. 19: George	Smith,	Kennedy Street, Parliamentary Road, Glasgow.
1871, Dec. 11: Hugh	Smith,	9 Kelvinside Terrace, North Kelvinside, Glasgow.
G. 1868, Dec. 23: M. 1874, Oct. 27: Hugh	Smith,	97 Wellington Street, Glasgow.
1878, May 14: James	Smith,	40 Margaret St., Greenock.
1870, Feb. 22: Edward	Snowball,	Engineer, Hyde Park Locomotive Works, Springburn, Glasgow.
1883, Oct. 23: Andrew	Sproul,	Palmerston Blds., Greenock.
1883, Dec. 18: Alex. E.	†Stephen,	12 Park Terrace, Glasgow.
John	†*Stephen,	Linthouse, Govan, Glasgow.
1881, Nov. 22: Alex.	Steven, (*Member of Council.*)	Provanside, Glasgow.
1867, Jan. 30: Duncan	Stewart,	47 Summer Street, Glasgow.
1874, Oct. 27: Peter	Stewart, (*Member of Council.*)	53 Renfield Street, Glasgow.

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G. 1873, Dec. 23: M. 1882, Oct. 24:	W. B.	Stewart,	1 Scotland Street, Glasgow.
1866, Nov. 28:	James	Stirling,	Loco. Engineer, S. Eastern Ry., Ashford, Kent.
Original:	Patrick	Stirling,	The Great Northern Railway, Doncaster.
1881, Jan. 25:	Walter	Stoddart,	Caledonian Railway, Carstairs.
1864, Nov. 23:	Edward	Strong,	°/₀ Kent Cottage, Queen's Cres., Southsea, Hants.
1877, Jan. 23:	James	Syme,	8 Glenavon Ter., Partick, Glasgow.
1879, Oct. 28:	James	Tait, C.E.,	Wishaw.
1882, Apr. 25:	Alex. M.	Taylor,	Java Cottage, Lenzie.
1879, Mar. 25:	Staveley	Taylor,	Russell & Co., Shipbuilders, Greenock.
1873, Dec. 23:	E. L.	Tessier,	Veritas Office, 29 Waterloo Street, Glasgow.
1882, Apr. 25:	Geo. P.	Thomson,	Clydebank Shipbuilding Yard, Glasgow.
1883, Dec. 18:	George	Thomson,	9 Buckingham Ter., Partick, Glasgow.
1874, Nov. 24:	Prof. James (*Member of Council.*)	Thomson, C.E., LL.D., F.R.S.S.L. & E.,	2 Florentine Gardens, Hillhead Street, Glasgow.
1868, Feb. 12:	James M.	Thomson,	36 Finnieston St., Glasgow.
1882, Mar. 21:	James R.	Thomson,	Clydebank Foundry, Glasgow.
1868, May 20:	John	Thomson,	36 Finnieston St., Glasgow.
1876, Feb. 22:	John	Thomson,	147 E. Milton St., Glasgow.
1875, Jan. 26:	Robert S.	Thomson,	19 Windsor St, Glasgow.
1864, Feb. 17:	W. R. M.	Thomson,	96 Buchanan St., Glasgow.
1878, May 14:	W. B.	Thompson,	Tay View, Broughty Ferry.
Original:	Thomas C.	Thorburn,	35 Hamilton Square, Birkenhead.

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1874, Oct. 27: Prof. R. H. Thurston, M.E., C.E., Steven's Institute of Technology, Hoboken, New Jersey, U.S.A.

1875, Nov. 23: John Turnbull, jun., Consulting Engineer, 255 Bath Street, Glasgow.

1876, Nov. 21: Alexander Turnbull, 14 Whitehill Terrace, Dennistoun, Glasgow.

1876, Jan. 25: Henry Turner, Managing Engineer, Canada Works, Birkenhead.

1880, Apr. 27: John Tweedy, Neptune Works, Newcastle-on Tyne.

1880, Nov. 2: Ralph H. Tweddell, 14 Delahay Street, Westminster, London.

1865, Apr. 26: W. W. Urquhart, Blackness Foundry, Dundee.

1883, Jan. 23: Peter Wallace, 25 Argyle Place, Partick, Glasgow.

1875, Mar. 23: G. L. Watson, 108 W. Regent St., Glasgow.

1864, Mar. 16: W. R. Watson, 16 Woodlands Ter., Glasgow.

1883, Jan. 23: D. W. Watt, 193 Sauchiehall St., Glasgow.

John *Weild, Underwriter, Exchange, Glasgow.

1874, Dec. 22: George Weir, M.E., 18 Millbrae Cres., Langside, Glasgow.

1874, Dec. 22: James Weir, M.E., Silver Bank, Cam'uslang, near Glasgow.

G. 1876, Dec. 19; M. 1884, Feb. 26; Thomas D. Weir, C.E., 97 W. Regent St., Glasgow.

1869, Feb. 17: Thomas M. Welsh, 63 St. Vincent Cres., Glasgow

1868, Dec. 23: Henry H. West, 13A Exchange Buildings, Liverpool.

1883, Feb. 20: Richard Saxton White, Shipbuilder, Sir Wm. Armstrong, Mitchell, & Co., Newcastle on-Tyne.

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1876, Oct. 24: Francis W.	Willcox,	45 West Sunniside, Sunderland.
1883, Feb. 20: Robert	Williamson,	Lang & Williamson, Engineers, &c., Newport Mon.
1878, Oct. 29: Thomas	Williamson,	3 Stella Pl., Pollokshields, Glasgow.
1874, Feb. 24: Alexander	Wilson,	Engineer Surv., Harbour Office, Melbourne.
Alex. H.	*Wilson,	Aberdeen Iron Works, Aberdeen.
1868, Dec. 23: James	Wilson, C.E.,	Water Works, Greenock.
1863, Mar. 18: James E.	Wilson,	1 Ranfurly Place, Paisley Road, Glasgow.
1870, Feb. 22: John	Wilson,	Wellfield House, Springburn, Glasgow.
1858, Jan. 20: Thomas	†*Wingate,	Viewfield, Partick.
1880, Nov. 2: Alexander	Wood,	8 Stockwell St., Glasgow.
G. 1873, Dec. 23: / M. 1884, Jan. 22: } Robert	Wyllie,	Hartlepool Engine Works, Hartlepool.
1879, Oct. 28: John	Young,	Phœnix Iron Works, Glasgow.
1867, Nov. 27: John	Young,	Galbraith Street, Stobcross Glasgow.

ASSOCIATES.

Thomas	*Aitken,	8 Commercial Street, Leith.
Andrew	*Armour,	63 Anderston Quay, Glasgow.
1883, Oct. 28: John	Barr,	Secretary to Glenfield Co., Kilmarnock.

Names marked thus * were Associates of Scottish Shipbuilders' Association at Incorporation with Institution, 1865.

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	T. S.	*Begbie,	36 Walbrook, London, E.C.
1882, Dec. 19:	Wm.	Begg,	47 West Cumberland Street, Glasgow.
1882, Jan. 24:	John	Black,	4 Alexandra Terrace, Govan Glasgow.
	Capt. Alex.	*Blackwood,	Leith.
1876, Jan. 25:	John	Brown, B.Sc.,	11 Somerset Place, Glasgow
1877, Nov. 20:	Capt. Wm.	Brown,	15 St. Vincent Pl., Glasgow.
1865, Jan. 18:	John	Bryce,	Sweethope Cottage, N. Milton Road, Dunoon.
1880, Dec. 21:	John	Cassells,	56 Cook Street, Glasgow.
1870, Dec. 20:	Joseph J.	Coleman, F.C.S.,	45 W. Nile St., Glasgow.
1859, Nov. 23:	Arch. Orr	Ewing, M.P.,	2 W. Regent St., Glasgow.
1863, Mar. 18:	Robert	Gardner,	52 North Frederick Street, Glasgow.
1860, Jan. 18:	George T.	Hendry,	79 Gt. Clyde St., Glasgow.
1882, Oct. 24:	Wm. A.	Kinghorn,	Ardoch Lodge, Cambuslang.
1864, Dec. 21:	Anderson	Kirkwood, LL.D.,	7 Melville Ter., Stirling
1878, Oct. 29:	John	Langlands,	88 Gt. Clyde St., Glasgow.
1884, Feb. 26:	C. R.	Lemkes,	198 Hope Street, Glasgow.
1865, Dec. 20:	John Jex	Long,	12 Whitevale, Glasgow.
1880, Nov. 2:	George	M'Callum,	322 Castle Street, Glasgow.
1873, Feb. 18:	John	Mayer, F.C.S.	2 Clarinda Terrace, Pollokshields, Glasgow.
1874, Mar. 24:	James B.	Mercer,	Broughton Copper Works, Manchester.
	George	*Miller,	1 Wellesley Place, Glasgow.
1865, Dec. 20:	John	Morgan,	Springfield House, Bishopbriggs, Glasgow.

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1883. Dec. 18:	W. M'Ivor	Morison,	Mayfield Marine Place, Rothesay.
	James S.	*Napier,	33 Oswald Street, Glasgow.
	John	Phillips,	17 Anderston Quay, Glasgow.
1869, Nov. 23:	Capt. John	Rankine,	31 Airlie Terrace, Pollokshields, Glasgow.
1867, Dec. 11:	William H.	Richardson,	19 Kyle Street, Glasgow.
1882, Dec. 19:	Colin Wm.	Scott,	30 Buchanan St., Glasgow.
1876, Jan. 25:	George	Smith,	101 St. Vincent St., Glasgow.
	John	*Smith,	Aberdeen Steam Navigation Co., Aberdeen.
1880, Nov. 2:	John	Steedman,	103 Cathcart St., Glasgow.
	Malcolm M'N.	*Walker,	45 Clyde Place, Glasgow.
	H. J.	*Watson,	5 Oswald Street, Glasgow.
	T.	*Westhorp,	West India Road, London.
1882, Dec. 19:	John D.	Young,	162 Buchanan St, Glasgow.
	William	*Young,	Galbraith Street, Stobcross, Glasgow.

GRADUATES.

1882, Nov. 28:	William H.	Agnew,	70 Grant Street, Glasgow.
1880, Nov. 2:	James	Aitken,	19 Egerton St., Chester.
1880, Feb. 24:	George	Almond,	Belmont, Bolton-le-Moors, Lancashire.
1877, Nov. 20:	James T.	Baxter,	146 W. Regent St., Glasgow.
1883, Dec. 18:	Seymour H.	Beale,	Banbury, Oxon.

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1871, Feb. 21: W. S.	Beck,	246 Bath Street, Glasgow.
1883, Dec. 18: Ludwig	Benjamin,	47 Canonbury Square, London, N.
1882, Feb. 21: Alfred G.	Berry, jun.,	33 Carnarvon St., Glasgow.
1883, Mar. 20: Andrew S.	Biggart	Forth Bridge Works, South Queensferry.
1883, Dec. 18: David	Blair,	28 Rosevale Street, Partick, Glasgow.
1884, Jan. 22: George	Blair, jun.,	6 Alfred Terrace, Hillhead, Glasgow.
1884, Jan. 22: Henry	Blair,	Clutha Ironworks, Glasgow.
1880, Mar. 23: Alexander	Bowie,	M. Langlands & Sons, Porter Street, Liverpool.
1878, Dec. 17: Rowland	Brittain,	Westinghouse Brake Co., London, N.
1883, Apr. 24: Arthur R.	Brown,	9 Brighton Place, Govan, Glasgow.
1876, Jan. 25: A. M'N.	Brown,	Castlehill House, Renfrew.
1879, Feb. 25: Alex. T.	Brown,	6 Olrig Terrace, Glencairn Drive, Pollokshields, Glasgow.
1883, Dec. 18: Eben. H.	Brown,	Blackhill, *via* Parkhead, Glasgow.
1881, Jan. 25: Matthew T.	Brown, B.Sc.,	13 Berlin Ter., Glasgow.
1872, Oct. 22: Hartvig	Burmeister,	c/o Rahr & Raundrap, 14 Brown St., Manchester.
1876, Dec. 19: Lindsay	Burnet,	Moore Park Boiler Works, Govan, Glasgow.
1882, Dec. 19: William T.	Calderwood,	6 Smith Street, Hillhead, Glasgow.
1873, Dec. 23: David	Campbell,	Motherwell.
1882, Dec. 19: Hugh	Campbell,	18 Norwood Road, Victoria Road, Headingley, Leeds.
1883, Jan. 23: W. W.	Campbell,	357 New City Road, Glasgow.

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1884, Feb. 26: John	Cleland, B.Sc.,	Woodhead Cottage, Old Monkland.
1884, Feb. 26: Arthur S.	Clerk,	9 Carmichael St., Govan, Glasgow.
1881, Nov. 22: Alfred A. R.	Clinkskill,	1 Holland Place, Glasgow.
1884, Feb. 26: Alexander	Conner,	9 Scott Street, Glasgow.
1877, Dec. 18: James	Conner,	Carstairs Junction.
1884, Jan. 22: Alex. M.	Copeland,	Bellahouston Farm, Paisley Road, Glasgow.
1874, Feb. 24: Andrew	Corbett,	3 Dixon Avenue, Crosshill, Glasgow.
1880, Dec. 21: Sinclair	Couper,	2 White Hill Gardens, Dennistoun, Glasgow.
1880, Nov. 23: James M.	Croom,	Earle's Shipbuilding and Engineering Co., Hull.
1882, Feb. 21: Wm. S.	Cumming,	Blackhill, by Parkhead, Glasgow.
1882, Mar 21: Alex.	Cunningham,	176 Dumbarton Rd., Gl'gow.
1884, Jan. 22: James	Dalziel,	119 Sandyford Street, Glasgow.
1883, Apr. 24: Alexander	Darling,	Governor's House, Berwick-on-Tweed.
1881, Mar. 22: David	Davidson,	24 Dixon Avenue, Crosshill, Glasgow.
1883, Apr. 24: Wm. St. John Vincent	Day,	115 St. Vincent St., Glasgow.
1879, Oct. 28: Jonathan L.	Dean,	4 Hartington Road, Middlesbro'.
1883, Apr. 24: F.	Delmaar	37 Berkeley Terrace, Glasgow.
1883, Dec. 18: William	Denholm,	17 Rosevale Street, Partick, Glasgow.
1883, Feb. 10: Lewis M. T.	Deveria,	Mansfield Cot., Kilwinning.
1882, Oct. 24: Daniel	Douglas,	Earle's Shipbuilding Co., Hull.

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1880, Nov. 2: Geo. C.	Douglas,	Douglas Foundry, Dundee.
1882, Oct. 24: John F.	Douglas,	c/o Napier & Son, Lancefield Street, Glasgow.
1883, Oct. 23: Harry W.	Downes,	9 Carmichael Street, Govan, Glasgow.
1884, Jan. 22: William	Dunlop,	961 Govan Road, Glasgow.
1882, Dec. 19: A. Von	Eckermann,	91 Pollok Street, Glasgow.
1878, Jan. 22: James R.	Faill,	Craig-en-Callie, Ayr.
1882, Feb. 21: Albert E.	Fairman,	Bellevue Bank, Helensburgh.
1880, Dec. 21: Henry M.	Fellows,	2 Castle Ter., Dumbarton.
1883, Dec. 18: John James	Ferguson,	Walworth Ter., Glasgow.
1884, Jan. 22: Thomas G.	Ferguson,	14 Queen's Cres., Glasgow.
1881, Feb. 22: William	Ferguson,	Larkfield, Partick, Glasgow.
1881, Nov. 22: Charles J.	Findlay,	10 Belmont Cres., Hillhead, Glasgow.
1873, Dec. 23: E. Walton	Findlay,	Ardeer, Stevenston.
1883, Oct. 23: Duncan	Finlayson,	1 Osborne Place, Govan, Glasgow.
1874, Feb. 24: John	Fleming,	52 Dumbarton Road, Glasgow.
1869, Oct. 26: F. P.	Fletcher,	South Russell St., Falkirk.
1880, Dec. 21: Robert	Fulton,	8 Margaret Street, Glasgow.
1874, Feb. 24: James	Gillespie,	21 Cleveland St., Glasgow.
1882, Jan. 24: Arthur B.	Gowan,	3 Octavia St., Port-Glasgow.
1884, Feb. 26: Alexander	Gracie,	9 Great George Street, Hillhead, Glasgow.
1881, Dec. 20: Andrew	Hamilton,	2 Belmar Terrace, Pollokshields, Glasgow.
1874, Feb. 24: Archibald	Hamilton,	New Dock Works, Govan, Glasgow.
1881, Jan. 25: Stephen	Hands,	9 Minerva Street, Glasgow
1881, Feb. 22: James	Harvey,	Park Grove Iron Works, Paisley Road, Glasgow.

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1883, Feb. 20: David	Henderson,	11 Hayburn Crescent, Partickhill, Glasgow.
1882, Nov. 28: F. N.	Henderson,	11 Princes Terrace, Dowanhill, Glasgow.
1881, Oct. 25: Charles G.	Hepburn,	125 St. George's Road, Glasgow.
1882, Feb. 21: Wm. S.	Herriot,	Leonora, Demerara.
1881, Jan. 25: A. C.	Holms, jun.,	Hope Park, Partick, Glasgow.
1880, Dec. 21: Duncan	Houston,	43 Belgrave St., Glasgow.
1873, Dec. 23: Guybon	Hutson,	Kelvinhaugh Engine Works, Glasgow.
1883, Jan. 23: John A.	Inglis,	23 Park Circus, Glasgow
1877, Mar. 20: Alex. C.	Jameson,	Edinburgh University.
1873, Dec. 23: David	Johnston,	12 York Street, Glasgow.
1883, Feb. 20: Eben. E.	Kemp,	Overbridge, Govan, Glasgow.
1880, Jan. 27: Robert	Kerr,	109 Bath Street, Glasgow.
1880, Nov. 2: John	Kyle,	204 St. Vincent Street, Glasgow.
1882, Jan. 24: Andrew	Laing,	Glenavon Ter., Crow Road, Partick, Glasgow.
1883, Nov. 21: William R.	Lester,	2 Doune Terrace, North Woodside, Glasgow.
1880, Nov. 2: Patrick F.	M'Callum,	Tangye's Steel Co., Soho, Birmingham.
1881, Dec. 20: H.	M'Coll, jun.,	3 Dalmeny Terrace, Pollokshields, Glasgow.
1883, Dec. 18: Peter	M'Coll,	Stewartville Place, Partick, Glasgow.

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1876, Oct. 24: Jno.M.	M'Currich,	M.A., Engineer's Office, Liverpool St. Station, London.
1883, Dec. 18: John	MacDonald,	293 New City Road, Glasgow.
1874, Feb. 24: George	M'Farlane,	65 Gt. Clyde St., Glasgow.
1879, Jan 21: James	M'Farlane,	40 Claremont St., Glasgow.
1882, Oct. 24: James L.	Macfarlane,	Meadowbank, Torrance.
1883, Dec. 18: John Bow	M'Gregor,	22 Church Street, Partick, Glasgow.
1882, Dec. 19: Allan	M'Keand,	24 Royal Cres., Glasgow.
1880, Feb. 24: Neil	M'Kechnie,	31 Bank Street, Hillhead, Glasgow.
1881, Oct. 25: James	Mackenzie,	16 Kelvinhaugh St., Gl'gow.
1883, Jan. 23: Thos. B.	Mackenzie,	342 Duke Street, Glasgow.
1883, Feb. 26: Robert	M'Kinnell,	56 Dundas Street, S.S., Glasgow.
1876, Dec. 19: John	M'Kirdy,	21 St. James Square, Edinburgh.
1883, Dec. 19: Colin D.,	M'Lachlan,	53 Bentinck St., Glasgow.
1875, Dec. 21: Hugh	M'Lachlan,	5 Dowanhill Place, Partick, Glasgow.
1880, Nov. 2: Robert	M'Laren, jr.,	Eglinton Foundry, Glasgow.
1874, Feb. 24: Andrew	Maclean, jun.,	Viewfield House, Partick, Glasgow.
1882, Dec. 19: Peter	M'Lean,	Trafalgar Cottage, South Queensferry.
1874, Feb. 24: William	Maclean,	Viewfield House, Partick, Glasgow.
1875, Dec. 21: Allister	M'Niven,	Clutha Iron Works, Vermont Street, Glasgow.
1880, Jan. 27: G. Scott.	M'Ritchie,	Ramage & Ferguson, Leith.
1879, Oct. 28: Donald	M'Taggart,	48 Overnewton St., Glasgow.
1880, Nov. 2: Ivan	Mavor,	Wincomlee, Low Walker-on-Tyne.

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1882, Jan. 24: Robt. Alex.	Middleton,	20 Merryland St., Govan, Glasgow.
1880, Feb. 24: Robert	Miller,	1 Wellesley Place, Sandyford, Glasgow.
1883, Dec. 18: Charles W.	Milne,	7 Carmichael Street, Govan.
1880, Feb. 24: James F.	Mitchell,	3 Minerva St., Glasgow.
1881, Jan. 25: Ernest W.	Moir,	Forthbridge Works, South Queensferry.
1881, Oct. 25: John	Moir,	26 St. Hilda Street, Hartlepool.
1882, Feb. 21: C. J.	Morch,	Horten, Norway.
1882, Nov. 28: M. J.	Morrison,	8 Annfield Terrace, Partick, Glasgow.
1884, Feb. 26: Andrew	Munro,	629 Govan Road, Govan, Glasgow.
1878, May 14: Angus	Murray,	47 Kelvinhaugh Street, Glasgow.
1883, Dec. 18: James L.	Napier,	22 Salisbury Pl, Hillhead, Glasgow.
1884, Feb. 26: D. J.	Nevill,	352 St. Vincent Street, Glasgow.
1879, Nov. 25: Alex. R.	Paton,	Redthorn, Partick, Glasgow.
1884, Feb. 26: Matthew	Paul, Jun.,	Levenford Works, Dumbarton.
1873, Dec. 23: Edward C.	Peck,	Yarrow & Co., Poplar, London, E.
1881, Oct. 25: William T.	Philp,	284 Bath Street, Glasgow.
1883, Jan. 23: Anders	Rambech,	44 Harmony Row, Govan.
1883, Nov. 21: Hugh	Reid,	10 Woodside Terrace, Glasgow.
1884, Feb. 26: Walter	Reid,	90 Bellgrove St., Glasgow.
1882, Nov. 28: J. M'E.	Ross,	Ravensleigh, Dowanhill Gardens, Glasgow.

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1878, Dec. 17: James R.	Ross,	45 Scotland St., Glasgow.
1875, Dec. 21: James	Rowan,	22 Woodside Pl., Glasgow.
1879, Nov. 25: Charles G.	Russell,	Russell & Co., Workington.
1879, Nov. 25: Robert	Russell,	Berlin Terrace, 28 Leven St., Pollokshields, Glasgow.
1884, Mar. 25: J. B.	Sanderson,	15 India Street, Glasgow.
1879, Mar. 25: John	Scobie,	Culdees, Minard Rd., Partickhill, Glasgow.
1880, Apr. 27: Archibald	Sharp,	31 Morrison St., Glasgow.
1882, Oct. 24: John	Sharp,	5 Pembroke St., Glasgow.
1883, Jan. 23: Adolph U.	Sheldon,	91 Pollok Street, Glasgow.
1883, Dec. 18: George	Simpson,	13 Maxwell Street, Partick, Glasgow.
1877, Mar. 20: Nisbet	Sinclair, jun.,	43 Park Road, Glasgow.
1884, Mar. 25: Russell	Sinclair,	9 Lawton Street, Newcastle-on-Tyne.
1882, Nov. 28: Geo. H.,	Slight, jun.,	413 East India Road, London.
1882, Dec. 19: Einar	Sommerfeldt,	Akersmekverksted, Christiania, Norway.
1881, Nov. 22: John A.	Steven,	12 Royal Crescent, Glasgow.
1881, Jan. 25: William	Stevenson,	Wigham, Richardson, & Co., Low Walker, Newcastle-on-Tyne.
1873, Dec. 23: John	Stewart,	270 New City Road, Glasgow.
1875, Dec. 21: Andrew	Stirling,	98 Dumbarton Road, Glasgow.
1878, Jan. 22: Benjamin B.	Sykes,	Caledonian Railway Works, St. Rollox, Glasgow.
1880, Dec. 21: Stanley	Tatham,	170 Kent Road, Glasgow.
1883, Dec. 18: Lewis	Taylor,	2 Salisbury Ter, Hillhead, Glasgow.

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1882, Nov. 28: William	Taylor,	57 St. Vincent Cres., Glasgow.
1883, Apr. 24: Wm. R.	Taylor,	Lennox, Lang, & Co., 131 W. Regent St., Glasgow.
1880, Nov. 23: George	Thomson,	64 Sycamore Road, Handsworth, near Birmingham.
1874, Feb. 24: George C.	Thomson,	77 Hill Street, Garnethill, Glasgow.
1883, Dec. 18: Nicol	Thomson,	39 Kelvinhaugh Street, Glasgow.
1884, Feb. 26: William	Warrington,	23 Miller Street, Glasgow.
1881, Mar. 22: Robert	Watson,	318 St. Vincent St., Glasgow.
1880, Apr. 27: Robert D.	Watt,	Butterfield, Swire, & Co, Shanghai.
1875, Dec. 21: Richard G.	Webb,	Engineers' Office, N. B. Rly. Co., Edinburgh.
1878, Dec. 17: Robert L.	Weighton, M.A.,	R. & J. Hawthorn, St. Peters', New'tle-on-Tyne.
1884, Apr. 22: John	Weir,	c/o Messrs R. & W. Hawthorn, St. Peter's, near Newcastle-on-Tyne
1882, Nov. 28: Geo. B.	Wemyss,	272 Main Street, Glasgow.
1883, Dec. 18: John	Whitehead,	Glasgow Street, Hillhead, Glasgow.
1877, Jan. 23: Robt. John	Wight,	7 Berlin Place, Pollokshields, Glasgow.
1879, Oct. 28: William	Willox, M.A.,	Park Lonmay, Aberdeenshire.
1883, Jan. 23: John	Wilson	175 North Street, Glasgow.
1883, Dec. 18: David	Wood,	124 West Nile Street, Glasgow.

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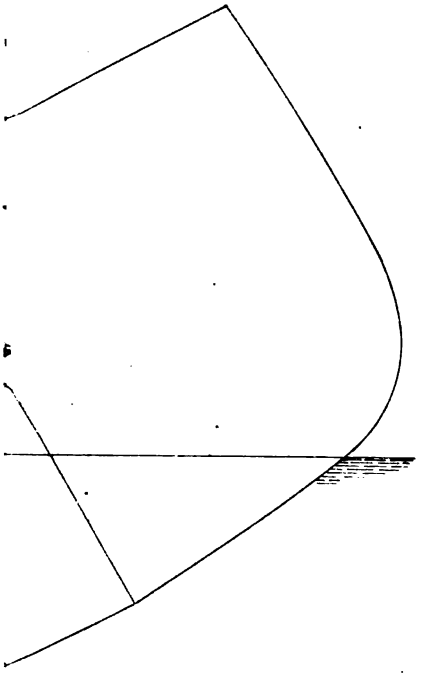

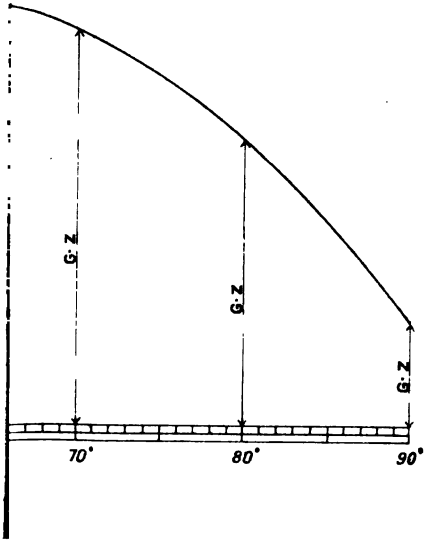

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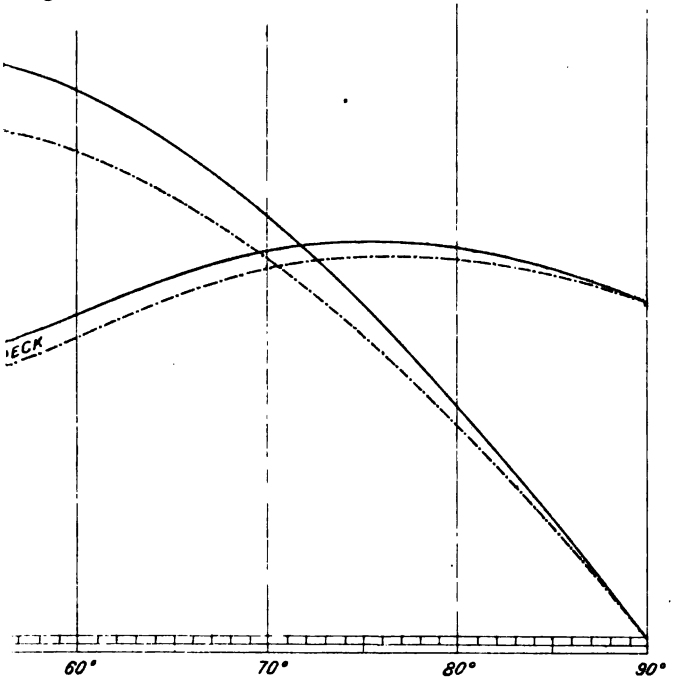

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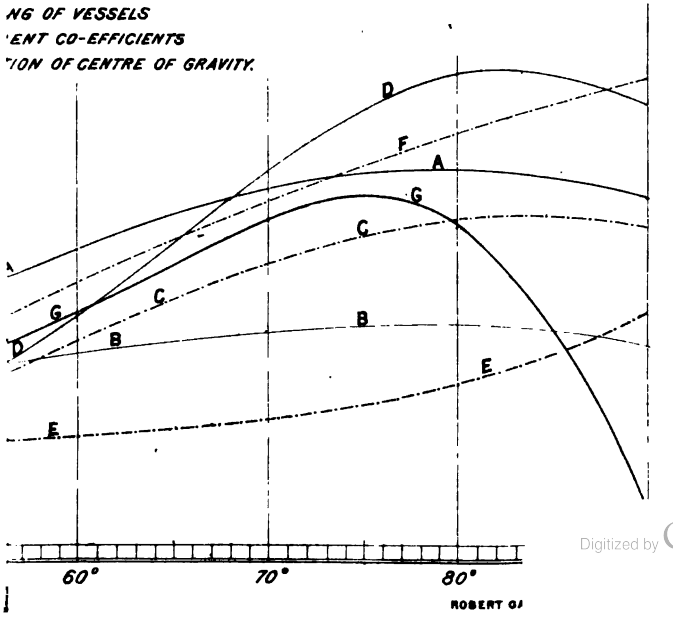

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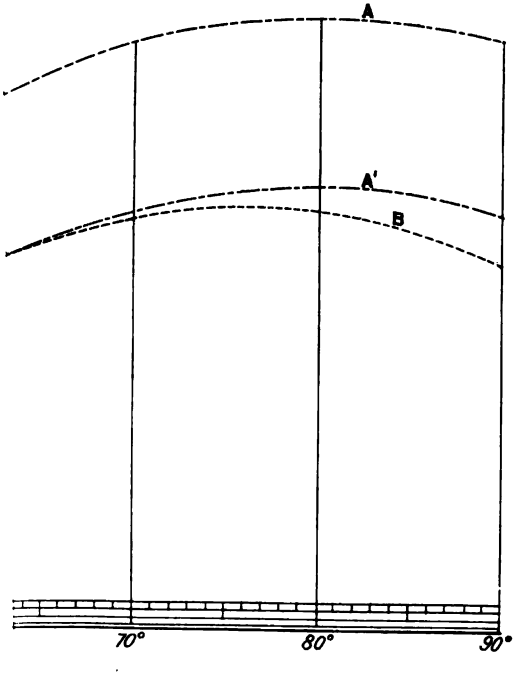

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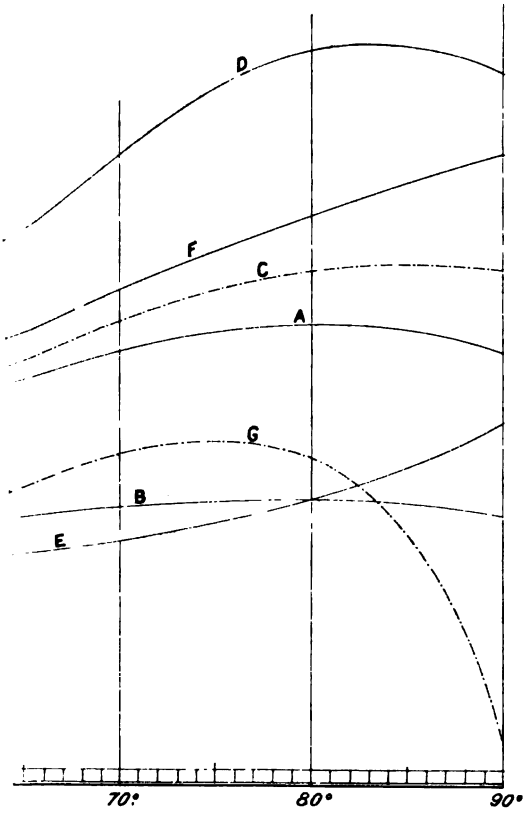

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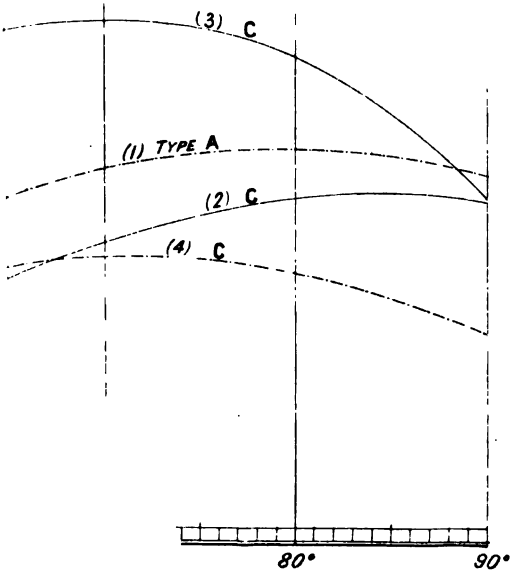

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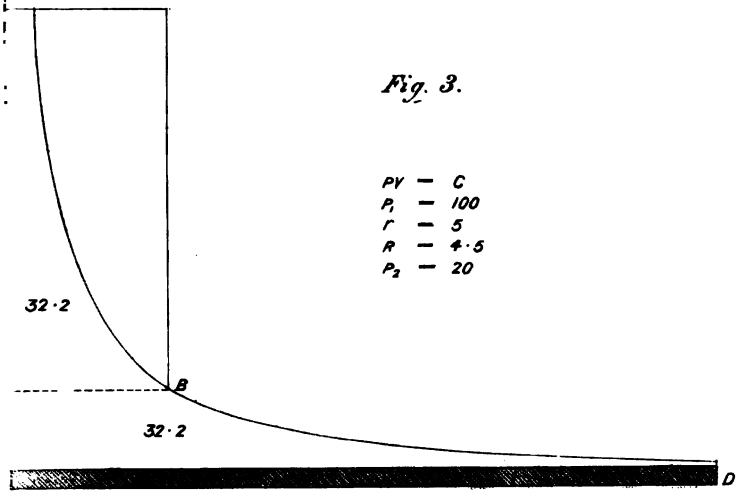

Fig. 3.

$PV = C$
$P_1 = 100$
$r = 5$
$R = 4·5$
$P_2 = 20$

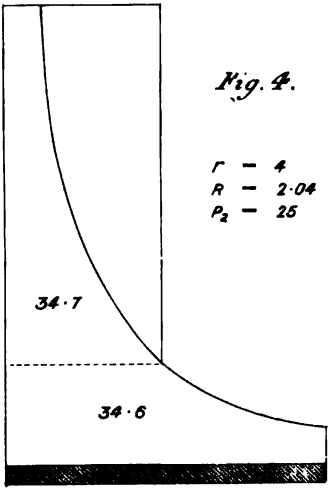

Fig. 4.

$r = 4$
$R = 2·04$
$P_2 = 25$

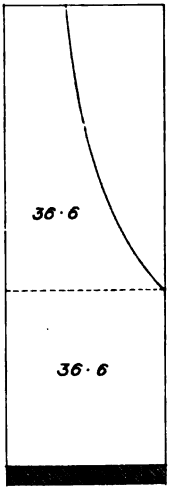

Fig. 5.

$r = 2·46$
$R = 1$
$P_2 = 40·6$

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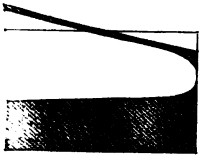

Fig. 12.

$PV^{\frac{8}{5}} = C$
$P_1 = 100$
$r = 3 - (2 \cdot 9)$
$R_1 = 3 \cdot 5 - (2 \cdot 9)$
$R = 3 \cdot 5$
$P_2 = 24 \cdot 8$

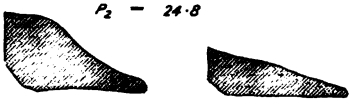

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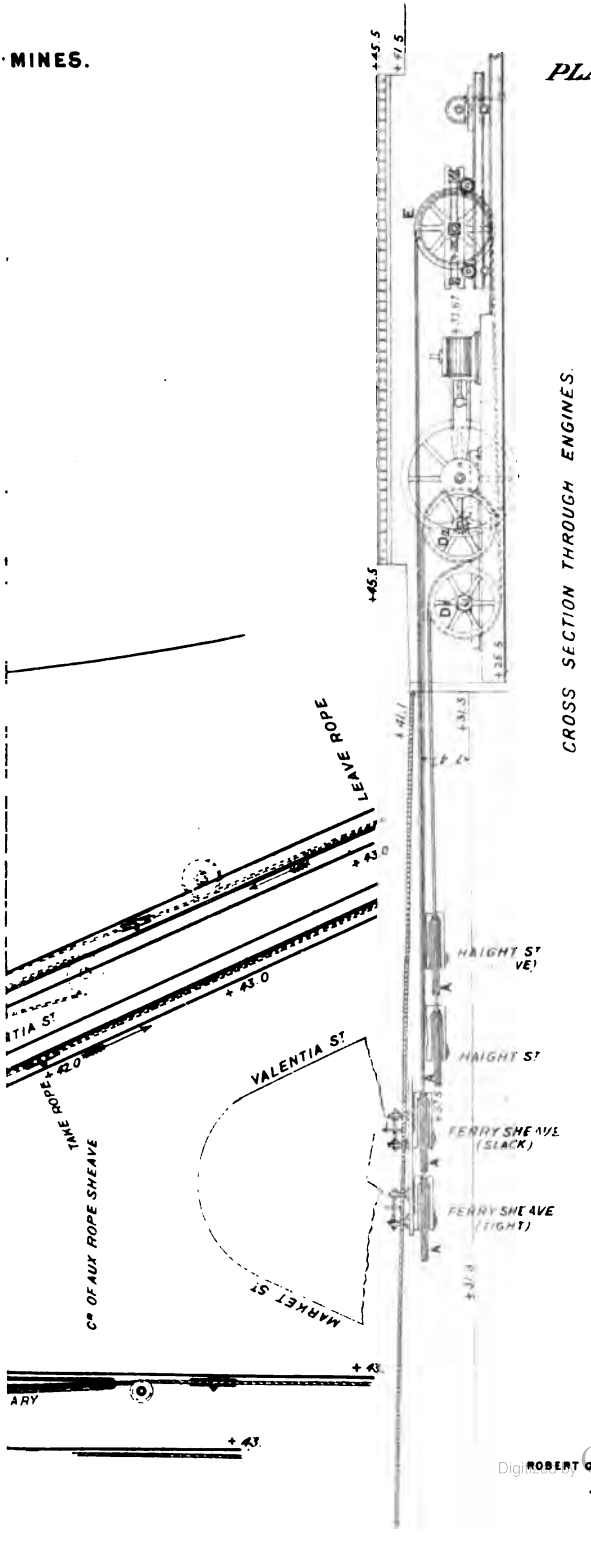
PLATE VI



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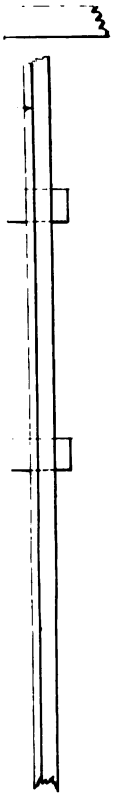

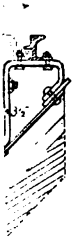

Fig. 3.

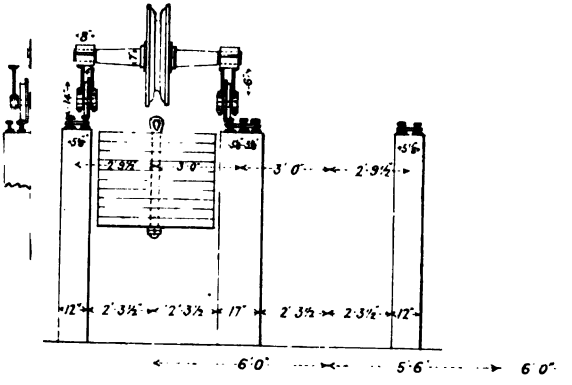

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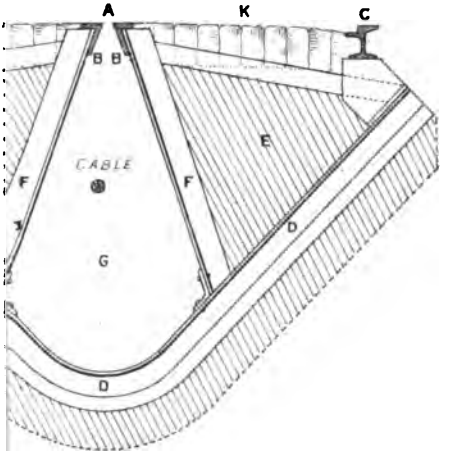
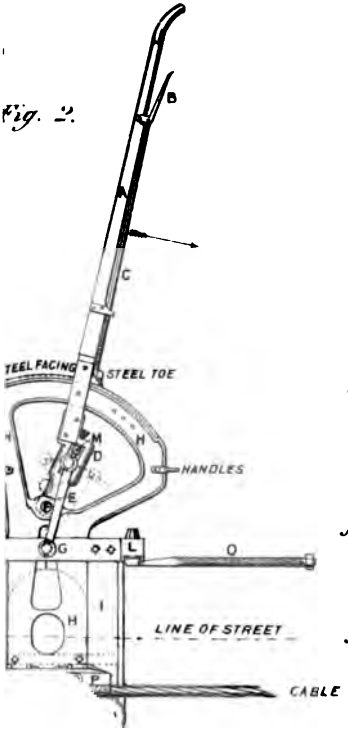
Fig. 1.

PLATE VIII



GRIPPING GEAR
USED ON
STREET CABLE RAIL ROAD,
SAN FRANCISCO, CAL.

Fig. 2.



A	*Lever.*
B	*Clasp.*
C	*Catch Bar*
D	*Toggle.*
E	*Connecting Links.*
F	*Toggle centre.*
G	*Stationary centre.*
H	*Quadrant.*
I	*Stationary Frame.*
J	*Idler Pulleys.*
K	*C.I. Pad for supporting rope*
L	*W.I. Strap connecting stationary frame to Dummy Floor.*
M	*Adjusting Screw in Toggle.*
N	*" " in Stationary Frame.*
O	*Connection to Draw Bar on Dummy*
P	*Box hinged to Quadrant carrying wooden Die for gripping rope.*

This Sketch shows grip closed on rope, to detach grip the Lever is unhooked and moved forward until centres are in dotted positions when the Quadrant will be lifted up 2 inches allowing rope to run without moving Dummy.

This grip cannot be used where the rope goes underneath another as in cross roads.

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4 5 FEET

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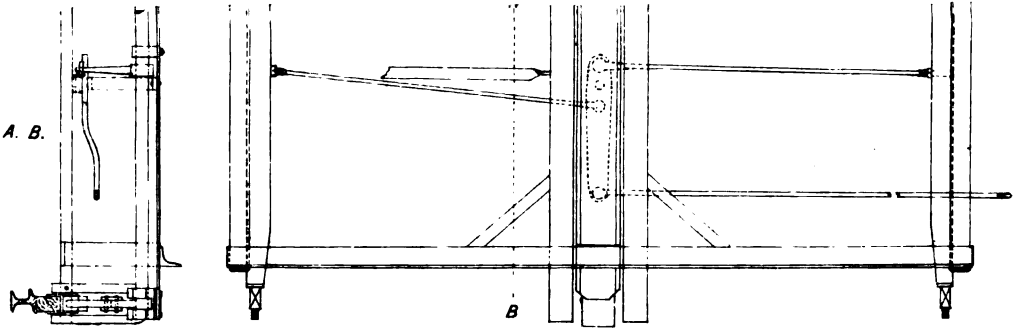
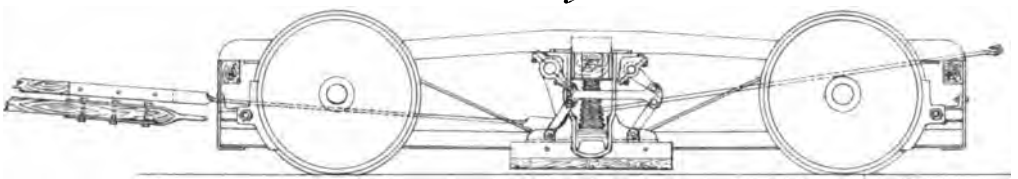


STIRRUP N.

Fig. 4.



REAR TRUCK.
Scale for Figs. 3 & 4.

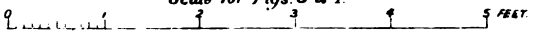


PLATE IX

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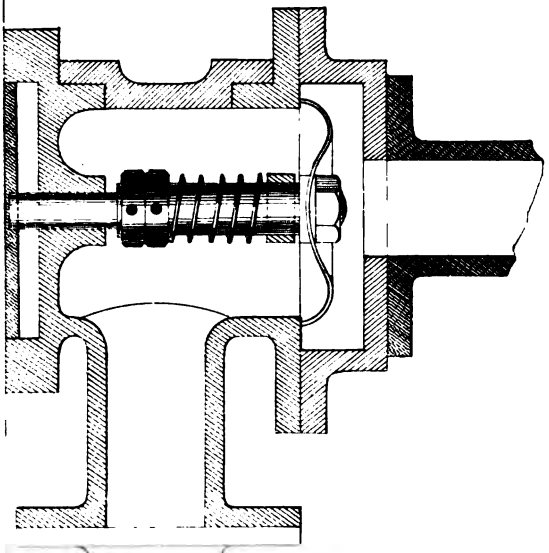

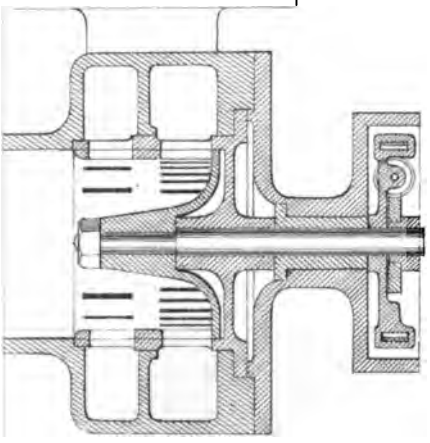

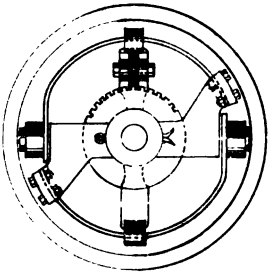

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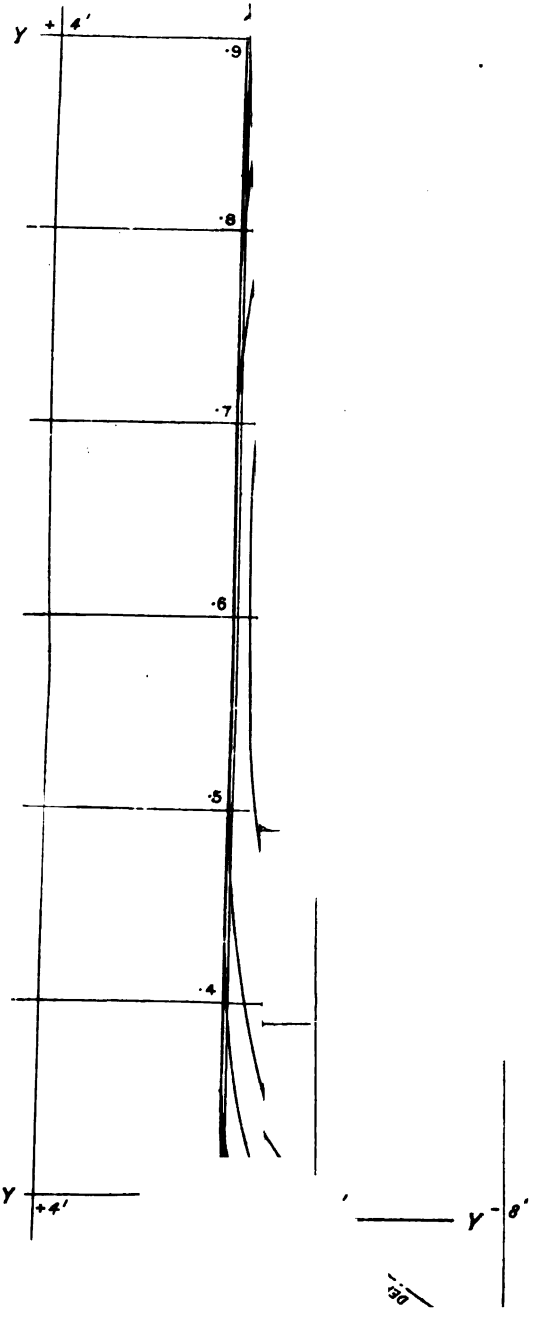
OF STABILITY FROM

P. PURVIS AND B. M

PLATE XI



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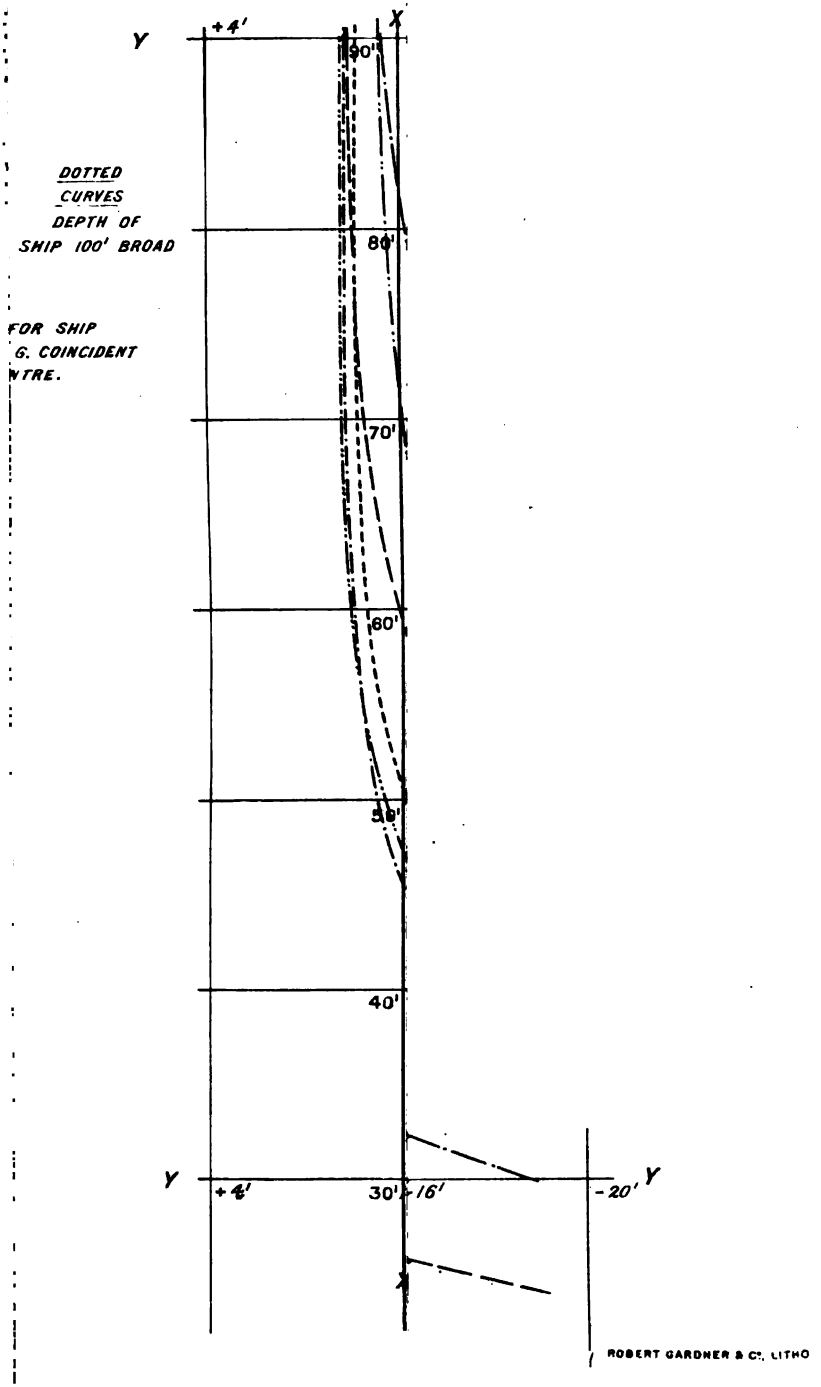
LITY FROM DATA FOR KNOWN
S AND B. KINDERMANN.

PLATE XII



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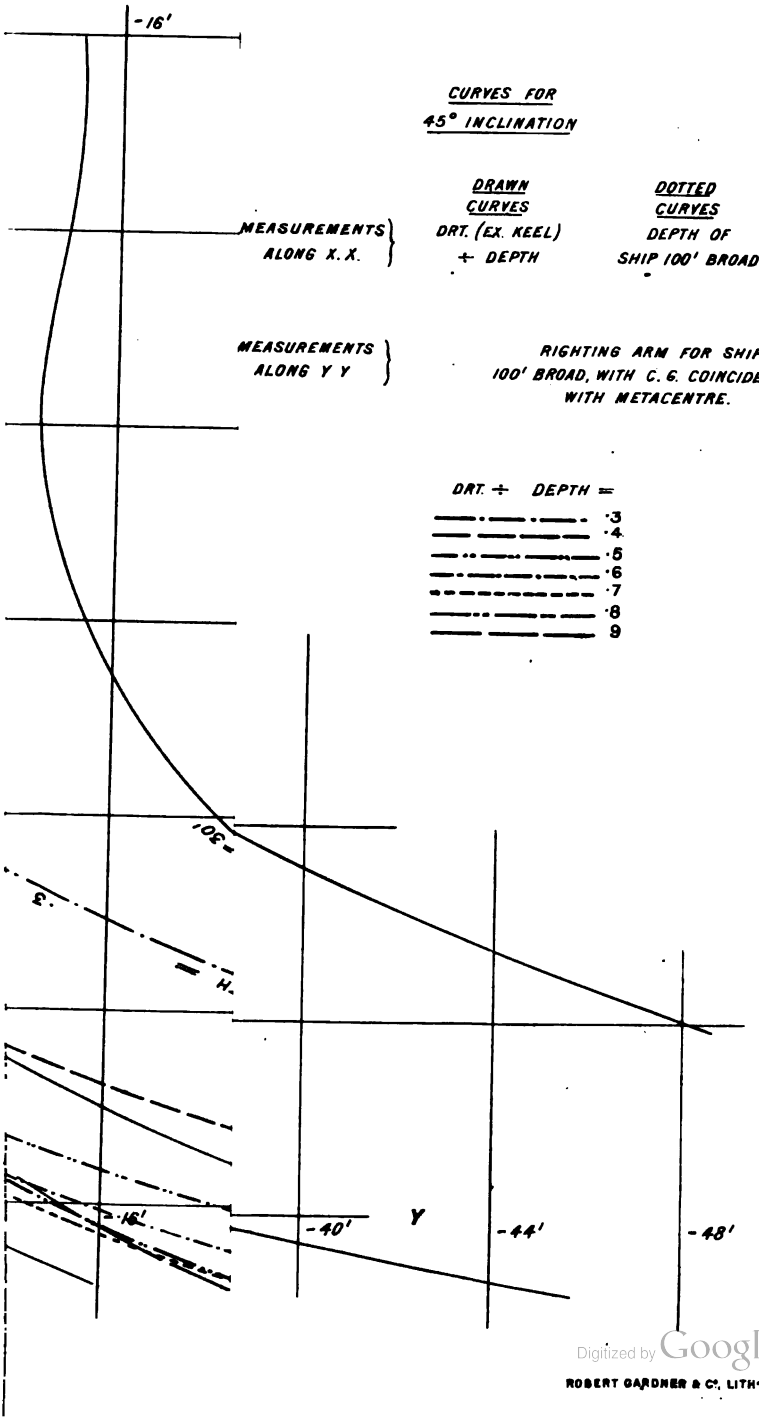
)XIMATION TO CUR

BY MESSR

PLATE XIII.



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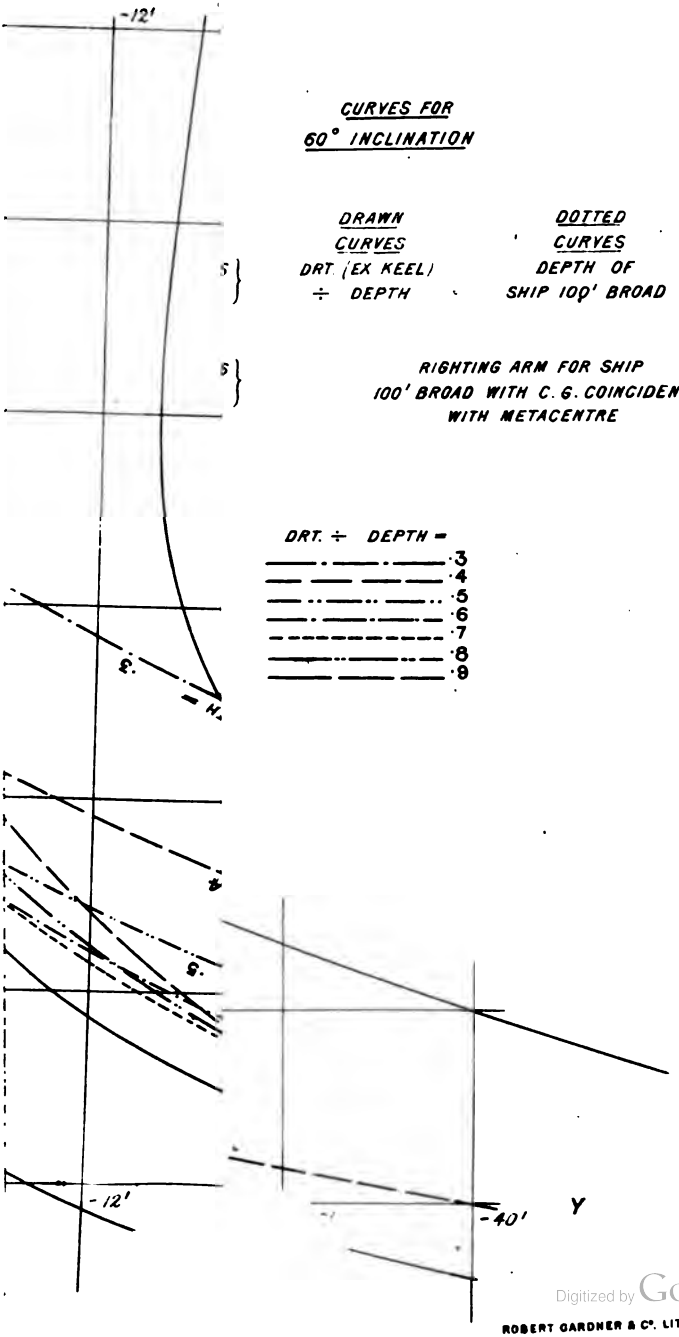
TO CURVES O

Y MESSRS. F. H

PLATE XIV



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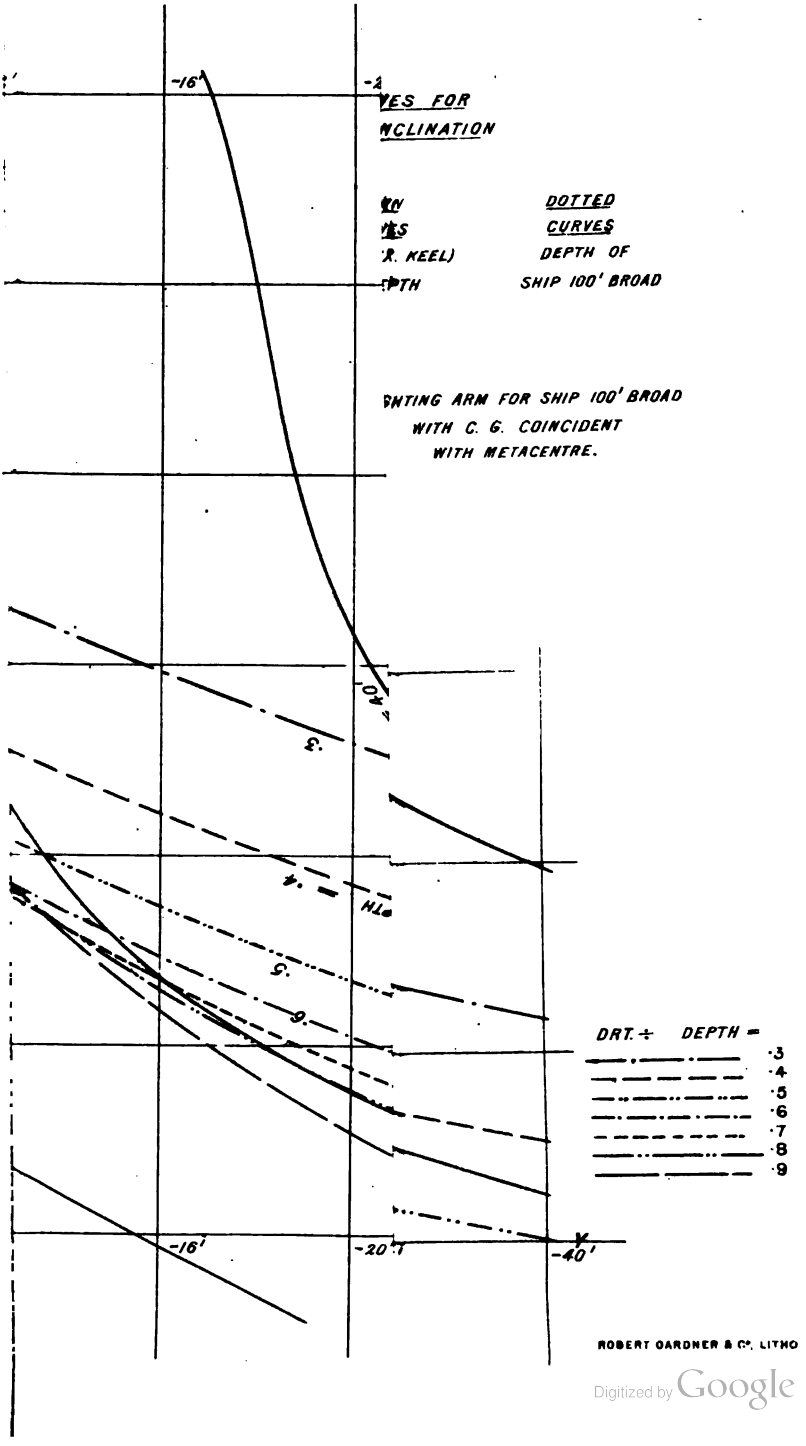
STABILITY FROM DATA FO
PURVIS AND B. KINDERMA

PLATE XV



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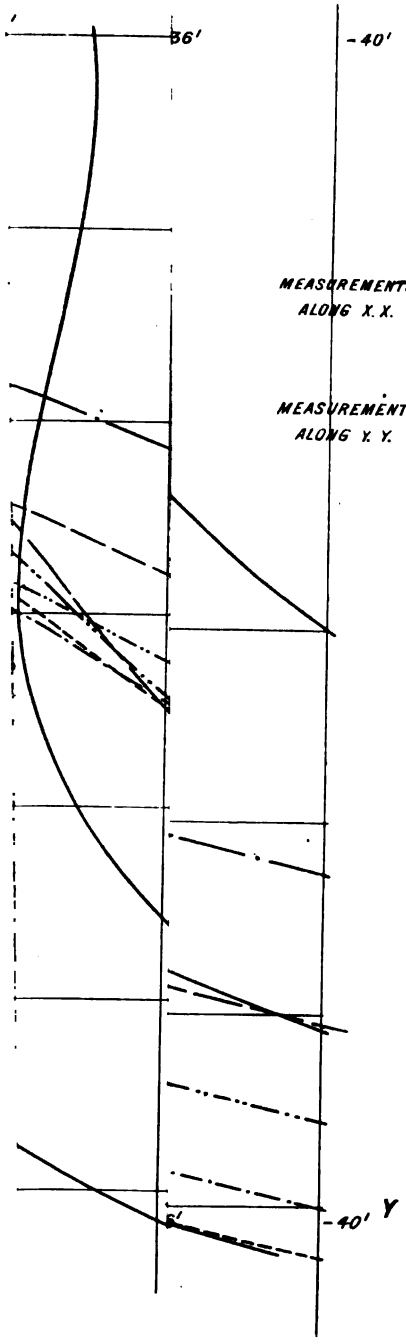
PPROXIMATIO

PLATE XVI



CURVES FOR 90° INCLINATION

MEASUREMENTS ALONG X. X.

MEASUREMENTS ALONG Y. Y.

DRAWN CURVES DRT. (EX. KEEL) ÷ DEPTH

DOTTED CURVES DEPTH OF SHIP 100' BROAD

RIGHTING ARM FOR SHIP 100' BROAD WITH C. G. COINCIDENT WITH METACENTRE

DRT. ÷ DEPTH =

·3
·4
·5
·6
·7
·8
·9

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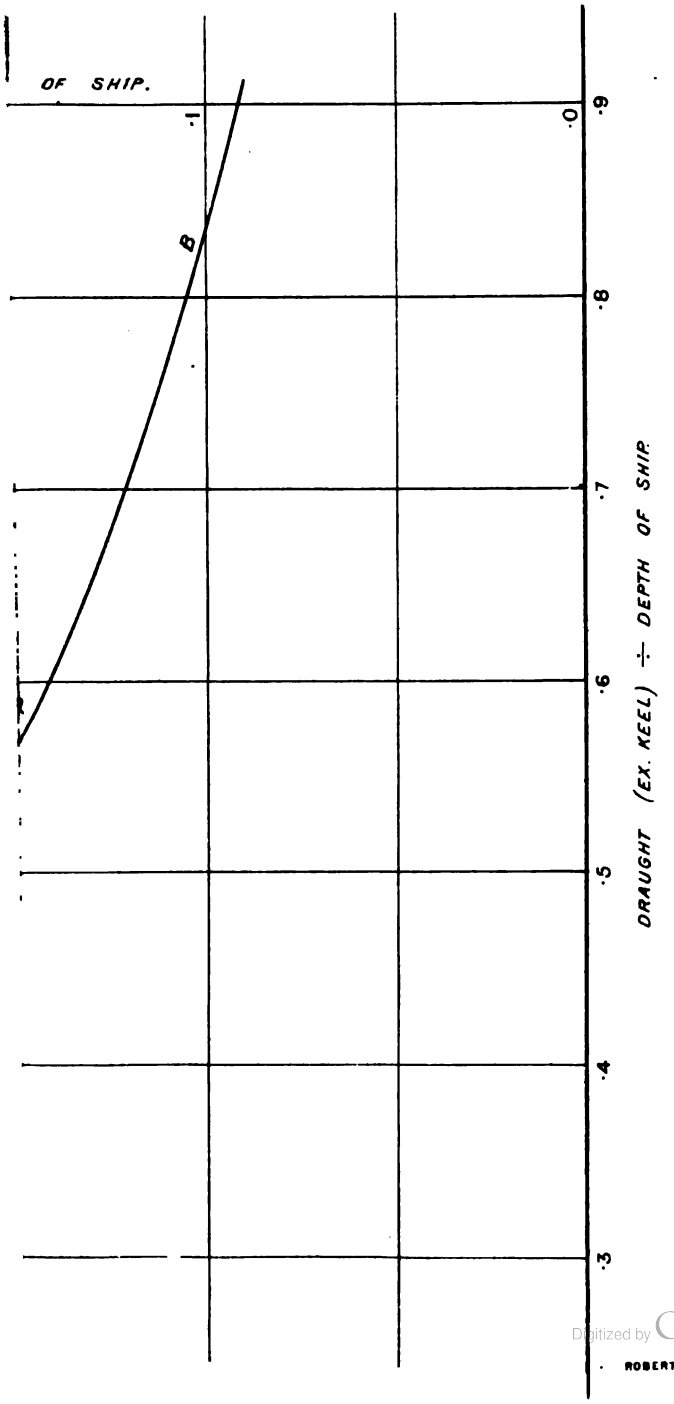
PLATE XVII



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